DOCUMENT RESUME

ED 141 130 SE 022 635

AUTHOR	Lomask, Milton
TITLE	A Minor Miracle. An Informal History of the National Science Foundation.
INSTITUTION	National Science Foundation, Washington, D.C.
REPORT NO	NSF-76-18
PUB DATE	76
CONTRACT	NSF-C-762
NOTE	290p.
AVAILABLE FROM	Superintendent of Documents, U.S. Government Printing Office, Washington, D.C. 20402 (Stock Number C38-000-00288-1, $2.75)
EDRS PRICE	MF-$0.83 HC-$15.39 Plus Postage.
DESCRIPTORS	*Administrative Agencies; *Federal Government; History; *Research; Resource Materials; *Science Education; *Science History; Scientific Enterprise
IDENTIFIERS	*National Science Foundation

ABSTRACT

This document reviews the history of the National Science Foundation (NSF) from its conception in May, 1950 to 1975. Included are reports on educational and institutional programs and science projects supported by NSF, biographical data of NSF directors and other prominent personnel associated with NSF, and the policy guidelines which have guided the foundation's directions. (SL)

****
* Documents acquired by ERIC include many informal unpublished *
* materials not available from other sources. ERIC makes every effort *
* to obtain the best copy available. Nevertheless, items of marginal *
* reproducibility are often encountered and this affects the quality *
* of the microfiche and hardcopy reproductions ERIC makes available *
* via the ERIC Document Reproduction Service (EDRS). EDRS is not *
* responsible for the quality of the original document. Reproductions *
* supplied by EDRS are the best that can be made from the original. *
****

ED141130

U S DEPARTMENT OF HEALTH,
EDUCATION & WELFARE
NATIONAL INSTITUTE OF
EDUCATION

THIS DOCUMENT HAS BEEN REPRODUCED EXACTLY AS RECEIVED FROM THE PERSON OR ORGANIZATION ORIGINATING IT. POINTS OF VIEW OR OPINIONS STATED DO NOT NECESSARILY REPRESENT OFFICIAL NATIONAL INSTITUTE OF EDUCATION POSITION OR POLICY

A Minor Miracle

An Informal History of the National Science Foundation

SE 022 635

NATIONAL SCIENCE FOUNDATION • WASHINGTON, D. C.

To persuade the Congress of these
pragmatically inclined United States
to establish a strong organization to
support fundamental research would
seem to be one of the minor miracles.

—Vannevar Bush

A Minor Miracle

An Informal History of the National Science Foundation

by Milton Lomask

NATIONAL SCIENCE FOUNDATION • WASHINGTON, D. C.

Foreword

To thousands of grantees and fellowship holders during the past quarter century, the National Science Foundation has been a source of welcome financial support, making possible good work that would not otherwise have been done. To some skeptics, it has been an absorber of funds that could have been better used for other purposes. Pluralists in the theory of government have considered it a valuable addition to the means by which science is given public support and sometimes used to help solve public problems. Centralists have periodically tried to use it as a nucleus for a larger and more comprehensive scientific agency. To the Office of Management and Budget it has sometimes been a frustratingly obstinate thwarter of efforts to use it as an administrative ally in reviewing the activities and budget requests of other Federal agencies engaged in research and development.

Clearly, one's appraisal of the operations and effectiveness of the National Science Foundation depends upon the perspective from which it is viewed and the interests and biases that determine that perspective. Thus an historian reviewing the Foundation's first quarter century of life could write any of several kinds of histories. One might write an organizational history, dwelling on the structure itself, its evolution, and the forces that shaped it. Or one could emphasize its financial growth, its competition for Federal funds, and the ways in which those funds have been used. Still another could focus on the scientific work it has chosen to support and its effectiveness in improving research, education, and the Nation's scientific facilities and competence.

Mr. Lomask's history is none of these in any complete sense, and yet a bit all of them. He calls it an "informal history," and in writing it he has picked and chosen those episodes and parts of the total record he thought would be of most general interest. He has included some of the legislative history and the debates and compromises that preceded the National Science Foundation Act of 1950; described the personalities and styles of the four men who have served as Director; reviewed some of the expanding responsibilities that changing circumstances have brought to the Foundation; and given an account of the changing but never settled controversy over its responsibility for the formulation of national science policy. All of these themes are illustrated with specific examples, some representing Foundation successes, and others Foundation failures or frustrations.

In the main, this is not a history of scientific work. The first chapter does include brief descriptions of a few examples of research supported by the Foundation, but they are no more than illustrations from a wide range of possibilities. Chapter 11 tells the story of one major project that was spectacularly successful at first, but that later degenerated into such strife that Congress finally said "No more" and cut off support. There is therefore some description of the kind of work for which the Foundation was established, but there is no attempt at a balanced appraisal of scientific achievement or of what the achievement has contributed to general human welfare. There could not be; as analyses supported by the Foundation have demonstrated, the time lag between most basic research and the practical innovations that draw upon that research is frequently as great as the Foundation's present age. For that kind of history we will have to wait for a later historian.

Political problems can develop more quickly, and sometimes have a shorter life span. One of the central issues of National Science Foundation history has been the definition of its role in the Federal structure and the nature of its responsibilities in formulating national science policy. The policy role took a new turn in 1973 when President Nixon abolished the Office of Science and Technology, the President's Science Advisory Committee, and the position of Special Assistant to the President for Science and Technology, and transferred much of their responsibility to the director of the Foundation. But these transferred responsibilities did not constitute new issues for the Foundation. In fact its structure and responsibilities were major points of argument from 1945 to 1950 when Congress was debating alternative NSF bills. President Truman vetoed the first NSF bill to pass both Houses of Congress because the planned structure and organization did not fit his standards and policies of Executive responsibility. And ever since it came into being the Foundation has experienced the common dilemma of a Federal agency established to deal with the problems of a particular constituency: How much should it serve the interests of its own constituency, and how much should it use its powers to further the objectives of the most central parts of the Federal Government. In this case, scientists generally wanted the Foundation to be *their* agency, while some other parties—most typically represented by the Office of Management and Budget—wanted it to be *their* aide and ally. The pulls in these two directions are the special topic of chapter 6, and an underlying theme throughout the Foundation's whole history and this account of that history.

The Foundation has been various things, but rarely have either its friends or its critics found it dull. It is still too young; its scope and its annual appropriations have grown too rapidly, and sometimes been curtailed too sharply; its influence on science has been too great; and it has been under too many watchful eyes to fall into dullness. Thus there is interest in its history from several points of view. To those who can remember its early years, this volume will revive memories of old battles—some won, some lost. To those who came upon the Foundation later, here is a readable and useful account of how it came to be the organization it now is.

Dael Wolfle
University of Washington

A Minor Miracle

1. Spotlight on Science 1
    The Geological Revolution 2
    Lighthouses in the Sky 7
    Analyzing the Economy 12
    Understanding the Immune System 16
    In the Footsteps of Einstein 20
2. How Firm a Foundation? 29
3. Old Fears and New Ideas 33
4. Choosing the Board and a Director 61
5. First Things First 71
6. To Coordinate or Not to Coordinate 91
7. The Early Directors 111
8. The Educational and Institutional Programs 121
9. Larger Ventures 137
10. Tempest in a Samovar 151
11. Project Mohole and JOIDES 167
12. Revising the Charter 199
13. New Faces and New Directions 215
14. RANN 237
15. The Fourth Director 251
16. New Dimensions 261

Index 281

Acknowledgments and a Word About the Book

It is always a pleasure to mention the people who have helped you in the preparation of a book if only because it indicates that the author has been keeping good company. In connection with this book I remember with special pleasure an hour spent with Vannevar Bush a few months before his death on June 29, 1974. At the time of my visit to his home in Massachusetts, Dr. Bush was 83. A recent operation had left him physically spent. In no way, however, had it diminished the sharpness of mind and force of character that made him one of the major architects of the partnership now in effect between the American Government and the American scientific community.

The process of collecting and clarifying the information needed for the book involved hundreds of letters, telephone calls, and interviews. It would be next to impossible to acknowledge all of the people who gave generously of their time and thought for this purpose. Among them were:

Alexander Abashian, Sophie D. Aberle, Vernice Anderson, William E. Benson, Lee Anna Embrey Blick, Detlev W. Bronk, Charles F. Brown, William D. Carey, Douglas S. Chapin, Mrs. Karl T. Compton, William V. Consolazio, John W. Davis, Frank D. Drake, Lee A. DuBridge, Gerald M. Edelman, Gerald A. Greenwood, Wayne R. Gruner, Philip Handler, Leland J. Haworth, Howard H. Hines, William J. Hoff, Daniel Hunt, Jr., Carlyle E. Hystad, Keith R. Kelson, Daniel J. Kevles, James R. Killian, Arthur J. Kusinski, Wassily Leontief, William D. McElroy, Senator Warren G. Magnuson, Howard A. Meyerhoff, Mary Ellen Mogee, Marston Morse, Francis G. O'Brien, Jack E. Oliver, Thomas B. Owen, Don K. Price, E. C. Reifenstein III, Aaron Rosenthal, Oscar M. Ruebhausen, David E. Ryer, Virginia B. Sides, Bernard Sisco, Maxwell M. Small, Joel A. Snow, Elmer B. Staats, David H. Staelin, John Roy Steelman, H. Guyford Stever, C. E. Sunderlin, Edward P. Todd, Mrs. Alan T. Waterman, James E. Webb, Joseph Weber, Jerome B. Wiesner, and John T. Wilson.

For calling my attention to pertinent information and in many cases for providing the appropriate documents or for assisting in their collection, I wish to say thank you to Bruce R. Abell, Raymond L. Bisplinghoff, Mary E. Bundy, Alma R. Burger, Marcel Bardon, Walter

H. Dodd, Eloise E. Clark, Elisabeth C. Creutz, Kenneth B. Foster, Florence Heckman, Elizabeth S. Hunt, Jack Kratchman, Emmett W. Lucas, Syl McNinch, Charles Maechling, Jr., Melvin Margerum, John W. Mehl, Marya Mikuluk, Patricia E. Nicely, William H. Pell, Frances M. Pentecost, Jack Renire, Alfred Rosenthal, Bertha W. Rubinstein, Barbara Tufty, Sir Vincent Wigglesworth, Carroll L. Wilson, Martha Jane Sordo Wilson, and James J. Zwolenik.

For reading all or parts of the manuscripts and for many useful suggestions: Vernice Anderson, William E. Benson, William D. Carey, H. E. Carter, Charles L. Drake, Frank D. Drake, Theodore D. Drury, Gerald M. Edelman, Estella K. Engel, Philip Handler, Leland J. Haworth, William J. Hoff, Daniel J. Kevles, Jack Kratchman, Wassily Leontief, William D. McElroy, Francis G. O'Brien, Jack E. Oliver, Joy Patterson, Rodney R. Porter, Don K. Price, E. C. Reifenstein III, Aaron Rosenthal, Alfred Rosenthal, Bernard Sisco, David E. Ryer, Maxwell M. Small, Philip M. Smith, David H. Staelin, James E. Webb, Theodore W. Wirths, and Dael Wolfle.

Finally I would like to express my special indebtedness to J. Merton England, the National Science Foundation historian, for assistance and encouragement extended steadily and unoppressively; to Dael Wolfle, whose Foreword opens the books, for sharing some of the insights flowing from his long and intimate association with the world of science; to Doris McCarn, personal secretary to all of the NSF Directors, for sharing her memories of an organization she has served since it was born; and to Mary Ann Flack and S. Justine Burton for typing numerous versions of the manuscript.

As the subtitle of the book suggests, this account of the first 25 years of the National Science Foundation is a selective one. It consists of episodes and developments chosen from a large number of possibilities in an effort to convey to the general reader the flavor of a unique public institution.

Allow me to add that, although this book was written under contract NSF-C762 to the National Science Foundation, the author alone is responsible for its interpretations and emphases.

Milton Lomask
McLean, Virginia, 1975.

Chapter 1

Spotlight on Science

The first yield from work in science is science—that is, objective knowledge.

> Gerard Piel. In Eugene V. Kone and Helene J. Jordan, eds., *The Greatest Adventure,* New York, 1974, p. 266.

. . . the scope of scientific research is as limitless as man's total ignorance of his environment.

> William D. Carey, "Science Administration," *Saturday Review,* Feb. 2, 1957, p. 45.

I want to know how God created this world.

> Albert Einstein. Quoted in Ronald W. Clark, *Einstein,* New York and Cleveland, 1971, p. 19.

. . . what Art was to the ancient world, Science is to the modern. . . . Rightly understood, Manchester is as great a human exploit as Athens.

> Benjamin Disraeli, *Coningsby,* 1844.

When the National Science Foundation first appeared on the scene 25 years ago, its name puzzled some people. "Foundation" had a private sound as in "Rockefeller Foundation" or "Guggenheim Foundation," but the National Science Foundation is an arm of the Federal Government, what is called an independent agency because it belongs to none of the large executive departments and functions on its own under the eye of the President. Its first operating budget, for fiscal year 1952, was $3.5 million. Last year it spent over $600 million. As such things are measured in bureaucratic Washington, this is progress. Yet even today, to the chagrin of a hard-working public information office, some people still ask, "What is the National Science Foundation?"

There are two ways of answering that question. One is to point out that NSF is the United States Government's all-purpose science organization. At least 40 other Federal agencies engage in scientific activity. Some of them—the Department of Defense, for example—spend billions on it against the Foundation's millions. But with minor exceptions these other agencies can pursue only those scientific ends that are clearly related to their statutory missions. What sets NSF apart is that, with even more negligible exceptions, it can sponsor any kind of science in any part of the country and, cooperatively with other nations, in any part of the rest of the world.

Another and perhaps more satisfactory way of identifying the Foundation is to examine some of the scientific activities it promotes. To proceed in this manner is to provide ourselves with a look at NSF as it now is. That done we can move back to its beginnings and trace its subsequent growth, observing as we do so some of the problems encountered and how the leaders of the agency have handled them.

Since the Foundation issued its first 28 grants in 1952, it has helped make possible well over 40,000 different scientific investigations. Countless reports in technical publications record the goals set, the methods used and the results obtained. In the limited space available here a few samples of work done with the aid of NSF funds in five different fields must stand for the whole.

The Geological Revolution

When during a series of lectures at the University of Oregon in 1970, geologist-oceanographer Charles L. Drake of Dartmouth College spoke of recent advances in the earth sciences as "the geological revolution," most of his American colleagues probably agreed with him.

In a preface to a printed version of the lectures, Dr. Alexander R. McBirney, chairman of the geology department at Oregon, had this to say:

"As part of man's increasing consciousness of his place in the natural world, geologists are beginning to achieve a dramatic new level of understanding of the processes that shape the face of our planet. Dr. Drake . . . calls recent advances revolutionary. Judging from their impact on nearly every aspect of geological research and teaching, this is not an exaggeration. How else can one describe a concept which promises to provide our first coherent explanation of the evolution of continents and oceans, mountain systems, volcanoes and earthquakes, climatic changes and even the distribution of ancient and modern forms of life?"[1]

Developed with astonishing rapidity during the 1960's, the concept mentioned by Dr. McBirney—the plate-tectonics model* of the earth—offers a picture of the planet we occupy that differs as much from the one more or less generally accepted in the late 1950's as the map of the world, after the discovery of America, differed from the one in use when Christopher Columbus left the shores of Spain for his first crossing of the Atlantic in August 1492.

One of the major elements of the theory is now known as continental drift. As the name indicates, continental drift postulates that the great continents of the world have moved and are moving. Their present position is transitory and was reached after a major splitting apart of a couple of mammoth land masses some 200 million years ago.

Obviously this intriguing notion was already under discussion by the late 18th century for on September 22, 1782, Benjamin Franklin was writing as follows to the French geologist, Abbe J. L. Giraud-Soulavie:

"Such changes in the superficial parts of the globe seemed to me unlikely to happen if the earth were solid to the centre. I therefore imagined that the internal parts might be a fluid more dense, and of greater specific gravity than any of the solids we are acquainted with, which therefore might swim in or upon that fluid. Thus the surface of the globe would be a shell, capable of being broken and disordered by the violent movements of the fluid on which it rested. . . . If [these thoughts] occasion any new inquiries and produce a better hypothesis, they will not be quite useless. You see I have given a loose to imagination, but I approve much more your method of philosophizing, which proceeds upon actual observation, makes a collection of facts, and concludes no further than those facts will warrant."[2]

For the next 150 years no facts adduced from any source were sufficiently unequivocal to warrant the conclusion that huge slabs of land can wander over the face of the earth. All this changed, however, in the early 1960's with the emergence of the hypothesis of sea-floor spreading originally put forward by the late Harry H. Hess of Princeton University. During the preceding years oceanographic exploration had revealed the existence along the center of the world's ocean basins of what Drake of Dartmouth has called "the longest single tectonic feature on the surface of the earth." His reference is to the 40,000-mile ridge that is now known to extend "through the middle of the Atlantic and into the Indian Ocean, where it splits, one branch going north to the Gulf of Aden and the Red Sea, the other continuing south of Australia into and across the Pacific Ocean."[3] Sea-floor spreading involves the belief that through cracks along this ridge, volcanic material (liquid basalt) wells up from the hotter region of the earth's mantle, creating new oceanic crust as it cools and moves away from both sides of the ridge in the direction of the opposite shores of the basin.

---

* The word "model," as used by scientists, rarely refers to a scaled-down replica of something. It signifies rather a picture or idea or system of ideas which qualified investigators have reason to believe not only represents something but also indicates how that something can be expected to behave under varying conditions. In this sense a model is a tool for making predictions and for the testing and refinement of theories.

Once this picture of a moving and constantly renewing ocean floor came into view, the geological revolution was underway. "And what a swift revolution it has been," geologist William E. Benson of NSF has been heard to exclaim. Swift indeed. By 1966 American students of the earth had tied the hypothesis of sea-floor spreading to the older idea of continental drift, thus bringing into existence the single unifying theme known as the theory of plate tectonics.

The essential assumption of plate tectonics, also spoken of as global tectonics, is that ours is a restless planet. Its outer shell (or crust) appears to be a mosaic of huge plates that move about, split apart, and collide. The earthquake belts of the world define the boundaries of the plates, each of which is 70 or more kilometers thick.

When two plates move apart, as happens along the great oceanic ridge, the liquid rock oozing up from the mantle below forms new plate material. When two plates collide, mountains may be thrown up. When they slide past each other a feature such as the San Andreas fault along the shores of California may be created, and at many convergent boundaries (ocean trenches) the crust is drawn down and recycled into the mantle (as much of the vast interior of the earth is known).

From this lively picture are we to conclude that the United States is even now moving away from Europe at the rate of perhaps a few centimeters a year? When that question is put to geophysicist Jack E. Oliver, he says that the answer depends on your point of reference. "Maybe not at this instant," he says, "but averaged over some longer period of time, this continent is surely moving west relative to Europe. Now how, or even whether, it's moving relative to some fixed axis in the center of the earth or something like that is still an open question."

On the faculty of Cornell University since 1971, Dr. Oliver was a professor of geology at Columbia University's Lamont-Doherty Geological Observatory when in 1964 an NSF grant helped him to inaugurate on the Tonga-Fiji islands of the South Pacific a series of studies of the earth's interior that have contributed in a substantial way to the development of global tectonics.

At the time these experiments began Oliver was no newcomer to the scientific frontier. While a graduate student at Columbia he had taken part in a geophysical exploration of the Arctic ice pack, arriving at the study area with the first group of U.S. planes to land on the pack for scientific purposes. In the late 1950's he discovered that surface waves generated by nuclear explosions can be detected at far greater distances than previously supposed. On one occasion he found in New York a wave of this sort that had been generated in Nevada. As the only person with knowledge of the phenomenon at that time, he soon found himself serving as a consultant to the negotiations in Geneva that eventuated in the adoption of the nuclear test ban.

He remembers following the early developments of the geological revolution with great interest—and crossed fingers. Hearing about Dr. Hess's sea-floor spreading ideas in the early 1960's, his first reaction, "like that of a lot of people," was simply "to sit back and watch" for a while. "I didn't really get convinced it was right," he says, "until it began to fit the data we were obtaining at Tonga-Fiji. That's a good way to be in science. Don't believe anything until it fits your data."[4]

To appreciate the relationship between the data gathered along the Tonga-Fiji island arc and the theory of plate tectonics calls for a consideration of some of the problems and questions that the theory raises. One of its widely held suppositions is that plate tectonics have directed the evolution of the terrestrial landscape for approximately 200 million years. By the same token sea-floor spreading has been going on for that long, which is the same as saying that for 200 million years new crustal material has been accumulating at the bottom of the oceans.

Has the earth grown larger as a result? Not much. "There is good evidence," writes geologist John F. Dewey of the State University of New York at Albany, "that the earth has not expanded more than about 2 percent in the past 200 million years."[5] Obviously this situation demands an explanation, and as the plate tectonics theory emerged its advocates set forth the assumption that when two plates override each other, one of them tends to sink into the earth. As it does so a huge ocean trench takes shape, and eventually the descending plate is assimiliated into (consumed by) the mantle. In other words, even as new plate takes form (at the rate of about a square mile per year) at the bottom of the sea, old plate appears to be disappearing at points spoken of as zones of subduction along the earthquake belts of the world.[6]

In the beginning this idea was theory. Largely as a result of the experiments in the South Pacific, it is now widely regarded as demonstrated. Since one way of exploring the earth's interior is to observe the behavior of waves generated by earthquakes, Oliver chose the Tonga-Fiji region for his studies. In no other area of the world do deep earthquakes occur with as much frequency. In 1964 he and a coworker, Dr. Bryan Isacks,* began the work there by setting up a network of seismograph stations. By 1967 they were able to propose "that beneath an island arc the suboceanic lithosphere [the earth's outer shell] bends over and descends into the upper mantle as a seismically active slab of low-attenuation and high-velocity material."[7]

Speaking of the early studies at Tonga-Fiji, Oliver points out that "at that time [1964] everybody thought that the mantle of the earth was laterally the same everywhere. What took Isacks and me to Tonga in the first place was a hunch. The hunch was that the great concentration of earthquakes in that zone could indicate that the mantle was peculiar there.

"Sure enough, we were able to show that it was. Frequently in science, and almost always in seismology, you have to work with differences that amount to only a percent or a few percent. That wasn't the case in the Tonga-Fiji region at all. The difference in the mantle there turned out to be in orders of magnitude. The evidence was extremely clear and definitive, and it led to the postulation that there was a downgoing slab of lithosphere there. That was a big result, a very happy result, one of the best things that's ever happened to me in science."

Having established the presence of a downgoing slab beneath the Tonga-Fiji island arc, Oliver and Isacks found themselves grappling with a question of research strategy. Had they "exhausted all the possibilities of the region?" Should they "now go elsewhere and do something different;" or should they remain where they were and study the processes that force the slab down and characterize its movements?

"We thought about that very hard," Oliver says. "and we concluded that there were more things to be learned in Tonga-Fiji by looking at the details."

The next important step was the discovery by Isacks that "in certain island arcs, at the level of the intermediate earthquakes, the downgoing slab was under extensional stresses oriented down the depth of the slab" whereas in other island arcs there were compressional stresses so oriented. "That was a clue," says Oliver, "that the slabs were definitely sinking, that they were more dense than the surrounding mantle, and that in the course of sinking they were pulling on the slabs above them and so creating extensional stresses. When the slabs reached a point far enough down, however, they encountered a layer of resistance and then showed compressional stresses in the lower part of the slab. Getting a line on the distribution of the stresses throughout the depth of the slab was another step really. It gave us some understanding of the mechanics that take the slab down there. Now we are trying to see whether there are more details about what happens to the slab that will tell us more about the process."

---

* Then with Lamont-Doherty, now at Cornell.

Of the geological revolution as a whole, Oliver says that "I think a lot of people now feel that we've made all the big discoveries. I don't feel that way. I think we've just opened a door and are going to find a lot of major new things in the very near future."

Such as? "Well, take the case of mineral deposits, for one thing," says Oliver. "Using the plate tectonics model as a base, I think we're really going to understand why minerals are concentrated where they are in the earth. That'll be a big thing economically. It's having a practical impact already. I think if you peek into any mining company or petroleum company today, you'll find geologists there working very hard on plate tectonics.

"Or take the matter of earthquakes. We have a project here [at Cornell] to study earthquakes that occur in normally aseismic areas like the eastern United States. Earthquakes in general have become more important to society because our support systems have become so complicated and vulnerable and affect so many people. You've got to take the possibility of an earthquake into consideration, for example, when you decide to build a large dam or a nuclear power plant. In our program here we're trying to integrate information about aseismic areas from seismology, geomorphology, sea level measurements, geodetic measurements, everything. And in the end plate tectonics will probably provide the basic framework in which we can relate these things in a way we've never been able to do before."

Lighthouses in the Sky

Students of the sky explore a region so vast and so full of curiosities that it would seem reasonable to assume that an astronomer would keep his cool even if he were to awaken some morning to find a unicorn nibbling on the grass of his lawn. As a matter of fact a wave of intense excitement coursed the astronomical world when in February 1968 a group of English investigators at Cambridge University announced that a strange class of radio-emitting objects, now known as pulsars, had been found in the distant recesses of space. A few months later Frank D. Drake—chairman of the Department of Astronomy at Cornell University and Director of Arecibo, to use the informal name of the National Astronomy and Ionosphere Center near Arecibo, Puerto Rico—pronounced the event "one of the most important and surprising scientific discoveries of all time."

Drake's description of a pulsar is a graphic one. It is a "sphere of pure nuclear matter," he writes, "smaller than the earth, emitting bursts of energy more powerful than the light of the sun at intervals more constant than the ticking of a clock."[8] One of several explanations of the way this sidereal oddity behaves ascribes the peculiar nature of its radiation to the presence of "a bright spot on a rapidly spinning star." As the spot "turns past our line of sight," it is perceived as a pulse. A pulsar, in other words, is "a kind of lighthouse in the sky."[9]

As this curious object is now thought to be a neutron star, its story can be said to have started some years ago. In 1939 three theoretical physicists—J. Robert Oppenheimer and George M. Volkoff in this country and Lev D. Landau in Russia—advanced the speculation that under certain conditions a celestial globe of gas as enormous as the sun (diameter: 865,000 miles) could collapse in such a manner that although the residual object, a neutron star, would be only some 10 miles in diameter it would be composed of matter so densely packed as to weigh 10 billion tons per cubic inch.[10]

By way of illustrating some of the properties of a neutron star, Drake offers an analogy. "We say around here [Cornell]," he says, "that if you converted all the water in nearby Cayuga lake [38 miles long and 1 to 3.5 miles wide] into neutron-star material and sprinkled it with white powder, you'd have a sugar cube. You could hold it in your hand. Not for long, of course, because it's billions of tons per cubic inch. But suppose you could, and suppose you dropped it. By the time it hit the floor it would have gained so much energy, it would go clean through the earth and come out on the other side."

The possibility that neutron stars existed, as originally expounded by Oppenheimer and others, was later elaborated on by a number of theoreticians. By 1966 some were suggesting that these objects could be born during the formulation of such well known phenomena as the Crab Nebula, the large bug-shaped oval of faint dust-like material first sighted in the sky by the Chinese in 1054 and now known to be the debris of an exploded supernova, the term applied to an aged star. Astronomers generally went along with this idea. For a variety of technical reasons, however, most of them were of the opinion that neutron stars could never be actually observed.[11]

Such was the prevailing thought on the matter when at Cambridge, England, in July 1967 astrophysicist Anthony Hewish* and his associates began a study of the flow of ionized gas from the sun known as the solar wind. Their discovery of pulsars was one of those happy accidents that scientists refer to as "serendipity."* Their newly constructed radio telescope had been in operation only a short time when a graduate student, Jocelyn Bell, noticed something out of the ordinary. Showing up on the chart she was monitoring was a "noise" unlike any cosmic signal ever recorded before. Among its more striking characteristics were the regularity with which the bursts came and went and the brevity of their duration. Impressed by the exact spacing of the signals, the Cambridge scientists briefly considered "the little green men" theory, the likelihood that they were receiving a message from intelligent occupants of another planet. This notion subsided as they found similar signals arriving from three other points in the sky and as it became apparent that they were not coming from planets but from stellar regions. But what sort of star could account for these clocklike pulses that ranged in duration from as little as one fourth of a second to 1.3 seconds?[12]

---

* Now co-holder with Sir Martin Ryle of the 1974 Nobel Prize in physics. The receipt of the prize by the two Cambridge scientists marked the first time it has been awarded to radio astronomers. Hewish was honored for his part in the discovery of pulsars, Ryle for the development of advanced techniques in the use of telescopes for the observation of detail in celestial bodies (*Washington Star-News*, Oct. 15, 1974).

Speculation on that question was underway at Cambridge when, after keeping the find a secret for 7 months, Hewish and his colleagues finally revealed the discovery of the first four pulsars. The effort to explain them quickly produced several theories. Offered in mid-1968 by astrophysicist Thomas Gold of Cornell, one of them was that the newlyfound objects were neutron stars. One of Gold's arguments was that only a celestial body of neutron-star size could rotate with sufficient rapidity to produce the effect of a measured beat that the Cambridge radio telescope had recorded. At first little serious attention was paid to Gold's idea. Then, in the fall of 1968, events at the two radio astronomy centers funded by the National Science Foundation yielded persuasive evidence that neutron stars and pulsars are essentially one and the same thing.

The first event came on November 6 with the revelation that at the National Radio Astronomy Observatory near Green Bank, W. Va. pulsar-type signals had been acquired from separate points in the sky by two young staff members, David H. Staelin of the MIT faculty and Edward C. (Ted) Reifenstein III, now with Environmental Research & Technology, Inc., a Concord, Mass., company started in the late 1960's by a group of MIT scientists. The announcement that both sets of signals obtained at Green Bank seemed to be coming from the vicinity of the Crab Nebula, an energetic supernova remnant, was regarded as of prime significance. It lent support to the already widely accepted belief that it is during the explosions responsible for supernova remnants that neutron stars are formed. Interest in what Staelin and Reifenstein had accomplished spread rapidly and one of their names seems to have suffered a sea change during a transatlantic telephone call, relaying the news to the Soviet Union. Report has it that a group of physicists at Moscow University were told, "Now America has a Stalin, and he has found a neutron star."[13]

---

* Coined in the 18th century by the London wit, Horace Walpole, the word "serendipity" alludes to *The Three Princes of Serendip*, a fairy tale whose heroes in the course of their travels are always finding things for which they are not looking. Serendip is an ancient name of Ceylon (William Rose Benet, ed., *The Reader's Encyclopedia*, New York, New York, 1948).

No sooner was the finding of the two signal sources at Green Bank made known than Frank Drake and the other Cornell specialists in charge of the radio astronomy observatory near Arecibo, Puerto Rico, began looking for them with the 1,000-foot bowl-shaped radio antenna that has been built into one of the natural sink holes of the cave-latticed limestone hills of that island. On November 18 the Arecibo searchers were able to announce that the source of some of the signals was definitely a pulsar and that it was located at the center of the Crab Nebula. Now spoken of as the Crab Pulsar, its discovery removed any lingering doubts concerning the identity of such objects as essentially neutron stars.

Originally supported by the Defense Department, now by the National Science Foundation, the mammoth antenna at Arecibo is one of the most powerful radio astronomy tools in the world. Working at Green Bank with the more limited 300-foot transit telescope there, Staelin and Reifenstein had been unable to determine the periodicities of the Crab Pulsar. The sensitive equipment at the Puerto Rico observatory showed the star to be pulsating at the amazingly swift rate of 30 times a second. It also demonstrated, beyond quibble, that the signals were not being produced by "intelligent life," although, as Frank Drake later remarked, "stupid life" could not be excluded.[14]

Although Staelin and Reifenstein are now giving their attention to monitoring the earth rather than astronomical objects, they still retain vivid memories of their labors in that field at Green Bank from October 1968 to March 1969. Both men smile broadly when asked if like the Cambridge investigators they were looking for something else when they "caught" the signals of the Crab Pulsar. "No," says Reifenstein, "we were looking for pulsars."

Taken together, the discoveries at Green Bank and Arecibo in 1968 may be regarded as the climax of the pulsar story. They did not, however, constitute its end. By 1974 more than 100 pulsars had been sighted. At least 30 of them had been scrutinized in detail or were being so observed, and much more information about them had been accumulated.

A talk with Frank Drake brings out some noteworthy facts and speculations about these lighthouses in the sky. One of the more interesting things now known is that with the passage of time a pulsar's rate of spin slows down. Drake points out that this change means that the star is losing energy. By measuring the rate at which the spin decreases, he says, observers can compute "to three or four decimal points" how much energy has been coming out of the pulsar. "What has become very clear," he adds, "is that the pulse that we see represents less than one percent of the energy actually emitted. It's as if you were studying a moving car. The bulk of the energy involved goes into pushing the car. In addition the car gives off some little squeaks and rattles. You can say that of the total energy radiated by a pulsar, all we are able to observe is the little squeaks and rattles. If we could bring to earth a cubic inch of the material of a swiftly spinning pulsar, it would take care of all our energy needs until the year 2000."

What happens to a pulsar as it dissipates more and more of its energy and slows down? Of the theories so far advanced to take care of that question, Drake briefly outlines the one he finds most convincing.

"A neutron star, a pulsar," he says, "is made when a large normal star collapses into a tiny object. At this point the star is extremely hot, 100 million degrees, and probably is radiating X-rays like crazy. It's also spinning very rapidly, but it spins slower and slower as it cools off by radiating X-rays. Finally it just gets slower and slower and colder and colder until it reaches a temperature lower than this room--and that's the end.

"Mind you, it never gets to absolute zero. It never stops altogether and it never will as long as the universe lasts. The point is it will never explode again or change into anything else.

"Space is full of these things. The oldest pulsars we've seen—oldest in the sense that we can see them radiating—are about 100 million years old. It looks as if all pulsars turn off, stop radiating pulses, stop squeaking and rattling when they're 100 million years old, which means we're only seeing the ones created within the last 100 million years. So there must be a hundred times more of them out there than we can see. It's just that they don't radiate anymore. They're just sitting there, cooled off; just slowly turning, once an hour or something, slowly turning."

A century and a half ago the sixth President of the United States, John Quincy Adams, won a bad laugh when during his inaugural address in 1825 he proposed that the Federal Government underwrite a string of astronomical observatories that he referred to as "lighthouses of the sky." Many voters decided they had put a fool in the White House when careless reporters quoted the new Chief Executive as speaking of "lighthouses *in* the sky." Such is the behavior of pulsars that Mr. Adams, were he with us now, would be entitled to the last laugh.

Analyzing the Economy

When a little more than 40 years ago Harvard University offered an instructorship to a 25-year-old Russian-born economist, Wassily W. Leontief, he accepted on one condition. The university must make it possible for him to continue his efforts to develop a new system for analyzing a national economy, a formula now usually spoken of as the Input-Output technique. The response of the Harvard authorities to the young man's request indicated no great faith on their part in the practicality of his ideas. They provided him with a research fund but stipulated that when this money was exhausted he was to report the "failure" of his inquiries.[15]

That was in 1931. When in 1973 Leontief received the Nobel prize in economics, the Royal Swedish Academy of Sciences cited him as "the sole and unchallenged creator of the Input-Output technique" and described the technique as "an empirically useful method to highlight the general interdependence in the production system of a society."[16]

Although use of the Input-Output model calls for mathematical computations of an intricate nature, the theory itself is relatively simple. Its basic principle is that every sector of an economy is both a producer of goods and services and a consumer of goods and services. The goods and services a sector produces are its output. Those it buys from other sectors of the economy are its input. The steel industry, for example, sells the products of its furnaces. Among the goods and services it obtains from other industries are trucks from the makers of motor vehicles, light and heat from utilities, and protection from governments.

Under the system of analysis devised by Professor Leontief the interrelationship of all outputs and inputs is graphically presented by means of a matrix that a newspaper reporter once likened to "a giant tic-tac-toe grid."[17] All sectors are listed down the side of the grid, one industry to a row. Precisely the same sectors are listed across the top of the grid, one industry to a column. Every number (a monetary value) shown in the cells of the grid is simultaneously an input and an output. To any sector listed at the left, each number in its row represents what it sells to one of the industries arrayed across the top of the grid—with the sum of these horizontally ranged numbers giving the total output of the sector named at the left. To each of the sectors along the top of the matrix, each of the numbers in the column below stands for the purchases it has made from one of the industries listed along the side—with the sum of these vertically ranged numbers representing the total input of the sector at the head of the column.

Leontief was not the first economist to dream of constructing a model that would show the movement of goods and services from one industry to another. Credit for that belongs to Francois Quesnay, the physician to King Louis XV of France and Madame Pompadour, who in 1758 published his *Tableau economique.* Quesnay's objective was to demonstrate the physiocratic contention that agriculture is the source of all wealth because the tiller of the soil feeds both the owners of the land and the *classe sterile,* as the physiocrats called the owners of the factories. Quesnay's data were fictional and some of his assumptions in error. His economic table, however, has been described as a three-sector precursor of the comprehensive picture of a whole economy that Leontief's Input-Output model can now provide.[18]

Since the story of Input-Output (I/O) covers both the formulation of a scientific concept and the steps taken to put it to practical use, it is really two stories in one.

To begin with, it is a striking case history of how advances in one field of scientific effort are often dependent on advances in other fields. Brilliant as the I/O theory is, it would still be little more than a challenging intellectual game were it not for other recent developments. One such development has been the emergence since Francois Quesnay's day of mathematical procedures sufficiently sophisticated to handle the complex equations involved in the application of the Input-Output theory. Another development has been the invention of the electronic computer, without which the assembling of the mountains of data required for a full-scale I/O matrix would be prohibitively timeconsuming.

In addition, the story of Input-Output illuminates the often crucial role that the character and judgment of human beings play in the process by which scientific discoveries are transmuted into technological innovations.

The results of a recent study undertaken for the National Science Foundation by Battelle Columbus Laboratories of Columbus, Ohio, cast considerable doubt on the once fairly common assumption that when a scientific discovery gives birth to a usable product or service, it does so as the result of a more or less automatic movement from discovery to application to development. Under this concept the term "application" describes the efforts of scientists and engineers to find ways of utilizing the discovery. "Development" includes the construction of a prototype and the solution of the problems involved in reproducing and marketing it on a commercially feasible scale.

Entitled *Interactions of Science and Technology in the Innovative Process: Some Case Studies,*[19] the Battelle report offers no objections to the picture of the innovative process as running from discovery via application to development. What the Battelle investigators do propose, however, is that the process is far from automatic and that more often than not the major factors required for its achievement are human beings. For the purpose of making their point the authors of *Interactions* employ a newly coined term, "the technological entrepreneur." They define the technological entrepreneur as any individual (or individuals) in an industry or other organization who, having perceived the practical values of a scientific discovery, persist in seeing to it that they are realized, often in the face of opposition from coworkers content with things as they are and anything but eager to subject themselves to the discomforts of change.

It is hardly surprising to find the development of Input-Output among the 12 case studies presented by the Battelle investigators. The authors of *Interactions* make it quite clear that I/O is now in wide use partly because of the refusal of its creator to be discouraged by recurrent setbacks, partly because of exertions on behalf of his theory by influential individuals in government and elsewhere. In short the story of Input-Output is to a large extent the story of its founding father and of those who recognized the potentialities of his model as a planning and marketing tool.

Leontief was born on August 5, 1906 in what is now Leningrad. His family was well off, thanks to the labors of a great-grandfather who was a serf before he moved to the city and established a textile business, and to those of a grandfather who became a prosperous industrialist and whom Leontief remembers as living with his family "and lots of servants" in a "Tolstoyan house" whose spacious chambers ("including the ballroom on the second floor") are now partitioned into apartments and occupied by several families. All of grandfather's sons "chose to be intellectuals," and Leontief's father was a professor of economics at the University of Leningrad.

By the time Leontief was ready to attend the university, the Russia of the tsars had become the Union of Soviet Socialist Republics. As an outspoken socialist he soon found himself arguing regularly with the Communist students around him. "They weren't bad folks really," he says. "The only thing was, when I got too vocal they'd have me clapped in jail. I'd go to jail, get released, study for a while, and be sent to jail again."

How then did he manage to leave the Soviet Union in 1925? Leontief's reply to that question is to point to an indentation in his neck. "It was a carcinoma," he explains. "Fortunately the Communists had not declared me a refugee. I still had a passport, so I got the permission of the authorities to go to Germany for treatment." The next time Leontief saw Leningrad it would be as an American citizen.

In Germany he learned that the tumor was benign. There, too, he continued his studies. By the time he received his Ph.D. from the University of Berlin in 1928, he had already joined the Economic Research Institute at the University of Kiel, where he remained until

1931, save for a 12-month period, beginning in 1929, during which he served as an economic adviser to the Nationalist government of Chiang-Kai-chek in China.

At Kiel he began working on ideas not unlike some of those that would later find their way into the Input-Output theory. Given Leontief's background it is easy to understand the occasional assertation that I/O was a strictly Russian development. Actually it was not. Most of the preliminary work on the theory was done in Germany and its author's first published paper on the subject appeared there. According to Leontief, the subsequent translation and republication of the paper in a Russian journal were undertaken without his prior knowledge and approval. It was during the years at Kiel that his work drew the notice of American specialists, and in 1931 he accepted an invitation to join the National Bureau of Economic Research in New York City. A few months later he began his long career at Harvard.

There he continued to work on his theory, undeterred by the assumption of some Harvard economists that his efforts stood small chance of success. Between the years 1932 and 1936 he not only developed the theoretical model but also constructed, with the help of a single assistant, the first Input-Output models of the American economy—as a matter of fact, of any economy--for the census years 1919 and 1929. This was a true breakthrough, but appreciation of Leontief's theory was slow in coming. His publication in the August 1936 issue of the *Review of Economic Statistics* of the first definitive exposition and example of it evoked no widespread interest. Finding this neglect of an important breakthrough hard to credit in retrospect, the authors of *Interactions* point out, as a possible explanation, that at that time the attention of American economists was concentrated on the recently publicized theories of John Maynard Keynes.* Apparently the situation was much the same 6 years later, for Leontief's first book-length treatment of his theory, published in 1941,[20] did not enjoy a substantial sale.

It did, however, attract the attention of officials of the United States Bureau of Labor Statistics. Their interest provided I/O with a battery of high-powered technological entrepreneurs whose faith in the theory proved well placed when, alone among the methods used by the Government to forecast economic conditions after World War II, Input-Output correctly predicted that there would be no depression as the result of demobilization.

---

* Keynes' first major work, *The General Theory of Employment, Interest and Money*, appeared in 1936. Its well known leading idea was that the classic concepts of supply and demand were obsolete and that the conditions of modern living require remote control of a national economy by a combination of monetary and fiscal policies.

By the later 1940's I/O had become a major instrument in the Government's efforts to guide the transfer of the American economy from a wartime to a peacetime basis. During this development members of the Bureau of Labor Statistics contributed several improvements to the theory. These were of such import that one knowledgeable observer has remarked that Leontief's "nerve and . . . vision" pioneered Input-Output analysis but it was "the BLS team. . .that made it work."[21] Among the BLS officials mentioned by the Battelle investigators were Duane Evans of the Productivity Division; Jerome Cornfield, a mathematical statistician in the Price Branch; and Marvin Hoffenberg, an expert on national income.

The authors of *Interactions* identify still another I/O technological entrepreneur in the person of Gerard Piel, publisher of *Scientific American*. They describe Piel as "an early advocate of widespread business use of I/O. . . ." Among other activities, *Scientific American* has published and distributed a series of large I/O tables for wall display and provided much of the initiative behind the formation of the Institute for Interindustry Data, Incorporated, a nonprofit association located in New York City and dedicated to improved business and governmental use of I/O analysis.

Today the construction of Input-Output tables is a major routine operation of government statistical offices, not only in this country but also in most other countries. The technique is also being used by the United Nations. During the 1930's work on I/O was privately supported. In the 1940's most of its funding came from BLS, and since 1961 Leontief has been the recipient of a series of National Science Foundation grants.

Understanding the Immune System

The award of the 1972 Nobel Prize for Physiology or Medicine to Dr. Gerald M. Edelman of Rockefeller University, New York City, and Dr. Rodney R. Porter of Oxford University, England, drew the attention of the public to a recent and promising advance in scientists' understanding of how the immune system of the human body develops and works.

In announcing the award, the Royal Caroline Institute of Sweden asserted that the revelations of its recipients had stimulated increased study in many laboratories of "all aspects of the science of immunology." This activity in turn was "yielding results of practical value for clinical diagnostics and therapy." Before Edelman and Porter began their research in the 1950's knowledge of the nature and mode of function of the key molecule of the immune system—the immunoglobulin known as antibody—had been sketchy. Working independently the two scientists had completed investigations whose findings, taken together, provided immunologists for the first time with an overall picture of the structure of the antibody molecule.[22]

As the name suggests, the immune system refers to those components of the body whose function is to defend it against infection and against the development of certain diseases. It is the immune system that operates to preserve the identity of the body when it is threatened by the intrusion of a foreign substance, a fact that makes the organization and behavior of the system matters of concern to surgeons called on to perform heart transplants and similar procedures.

The complexities attendant on the way the immune system accomplishes its missions have been compared with those of the nervous system. "Both systems," writes Niels K. Jerne, director of the Basel Institute of Immunology of Switzerland, "are diffuse organs . . . dispersed through most of the tissues of the body. In man the immune system weighs about two pounds. It consists of about a trillion (10^{12}) cells called lymphocytes and about 100 million trillion (10^{20}) molecules called antibodies. . . ." Produced and secreted by the lymphocytes, the antibodies do most of the work, putting underway the process by which other mechanisms available to the body rid it of bacteria, foreign corpuscles and other so-called antigens.

Antigens are molecules that bear on their surfaces specific patterns, variously spoken of as epitopes or antigenic determinants. As antibodies are also pattern-bearing molecules, they achieve their ends by recognizing antigens with complementary epitopes and by combining with them somewhat as differently shaped locks accept differently shaped keys.

The immune system is a constantly decaying, constantly renewing one. Every minute the body manufactures a million or so new immunoglobulins to replace those that have gone. "This might not be so astonishing," Jerne writes, "if all those antibody molecules were identical. They are not. Millions of different molecules are required to cope with the task of pattern recognition. . . ."

Obviously the immune system is highly selective. Of the trillions of antibodies on hand in the human body at any given moment, some will find antigens to attach themselves to and some will not. "An antibody," Edelman has remarked, "doesn't know its an antibody until it encounters its antigen. A lot of them just go to their death doing nothing for nobody, nowhere and never." A wasteful system, undeniably, but as Jerne writes, "Nature differs from technology in its approach to problemsolving. . . . For example, rather than improving the chance that a spermatozoon will meet an egg cell, nature finds it easier to produce millions of spermatozoa. . . ."[23]

The existence of antibodies was established in the 1890's but, as has been mentioned, information about them was still frustratingly vague when Edelman and Porter began their groundbreaking investigations in the 1950's. By then a series of papers on the subject, published in the early years of the decade, had already marked Dr. Porter as a pioneer in the science of immunology.

Twelve years the senior of his American colleague, Porter (55 in 1972) received his training as a chemist at the universities of Liverpool and Cambridge. He was a professor of immunology at St. Mary's Hospital School in London when in the late 1950's he found himself unable to obtain support for his studies in his own country and turned to the United States. For 6 years, beginning in 1960, almost all of his research funds were provided by the National Science Foundation.*

Since 1967 he has been Whitley Professor of Biochemistry at Oxford. Descriptions of him by English associates feature such expressions as "very serious," "somewhat intense," "shy and taciturn."[24] One is tempted to add "droll" in view of his response to a recent request from a staff member at NSF that he edit a tape-recording of one of his scientific talks. In his return letter, Porter pointed out that he had made changes in the transcript but that he did not regard the speech as of great scientific value. "Perhaps," he suggested, "you can leave it quietly in the files where it won't worry anyone."[25]

Edelman, by the time he came to Rockefeller University as a student in 1954, had done research in Britton Chance's laboratory at the Johnson Foundation of the University of Pennsylvania, had acquired an M.D. from that institution, and had served for 2 years as a doctor in the United States Army. Joining the faculty at Rockefeller in 1960, following the receipt of his Ph.D., he became professor of biochemistry there 6 years later. Colleagues speak of him as a man of "intense drive and energy," one of them calling him "Intellectually . . . one of the two or three most outstanding people I've ever met."[26] Other qualities become evident during the give and take of a conversation in his office: An urbane humor, for one, and a pronounced *savoir faire.*

There was a moment, some 15 years ago, when during a personal encounter in New York he and Porter briefly considered working together, only to abandon the idea for what one of them later spoke of as "geographical and family reasons." During the years that followed, however, they kept in touch, each of them making considerable use of the other's findings.

---

* Practically all of the support for Dr. Edelman's work with the antibody molecule has been provided by NSF.

Frequently their findings differed. One reason for this was that some of their basic assumptions differed, especially in the beginning.

The antibody molecule consists of many strung-together amino acids, and in the beginning Porter thought of it as a single chain. At Rockefeller, in 1959-61, however, Edelman and his associates found the molecule to be more complex. It consists of light and heavy chains, so designated because of their molecular weights. Following this lead, Porter and his colleagues in 1962-63 performed a series of experiments that demonstrated what is now the universally accepted four-chain model. One of Edelman's contributions was his perception that much could be learned about antibodies by studying a family of related substances known as Bence-Jones proteins for the English physician, Henry Bence-Jones, whose discovery of their presence in the urine of patients with multiple myeloma in 1845 yielded a new way of diagnosing that disease. Edelman and his student, Joseph Gally, showed that Bence-Jones' proteins were in fact homogenous light chains.

Another source of sometimes differing results was that the methods of analysis used by the two men were not the same. "The method I used," Edelman explains, "broke the bonds holding the light chains to the heavy chains, each of which is a natural synthetic unit—that is, the chains are made separately by the cell. The method Porter used cleaved the heavy chain into fragments that are not natural subunits. It's a combination of these two approaches that enables us to draw the structure. If you map one to the other the basic structure comes out."

Working from reports on the progress of Edelman and Porter in the *Journal of Experimental Medicine* and elsewhere, other scientists helped to put the results in order. By 1969 the chemical structure of an entire antibody molecule was determined completely by Edelman and his associates, and it was possible to make detailed comparisons of the molecule with the structure of a heavy chain determined by Porter and his associates.

It is only natural to wonder what scientific and practical effects an accomplishment of this magnitude can be expected to entail. Pointing out that it "seems a double burden to ask the guy who put down the theory of gasoline engines what the shape of next year's Chrysler's going to be," Edelman handles the question with understandable caution.

"The accomplishment," he says, "is quite comparable, I think, to the introduction of say, a periodic table into chemistry. Chemistry could function without a periodic table, but the table changed the way in which you looked at the entire subject and the efficiency with which you could improve an exterior idea. It was pervasive in that abstract sense, and I guess that's the problem. The problem is exactly that of asking, say, Jim Watson*, 'Well, you've discovered the double helix, but what has that got to do with this or that disease? And the answer is, 'Something—but what are you asking me for? Every time you turn around you're going to have to come back to DNA.'

"Similarly, a kind of rationalization was introduced into the field as the result of this antibody work. To get more explicit, it's fairly obvious that you can say once you know the structure of the thing, you can see myriad ways in which it might influence diagnostic routines or therapeutic routines, but you have to be a little double iffy. You have to say, 'If a certain diagnostic routine required an antibody with only one combining site, we can now make you one. If you want to remove one part of the molecule because it's deleterious, we can carve it out with exquisite precision. But what's your idea?'

"There's a certain thing in science called readiness; a good scientist knows when he's ready. He says, 'I refuse to think about charge; we're not ready yet. I'll think about mass.' Or he says, 'I can't think about antibodies, we don't have the equipment, but I can deal with antigens.' Until recently that cellular problem just wasn't ready. As a result of the antibody thing plus the independent efforts of the cellular people, it's now ready.

"Here we've been working on methods of separating cells according to the antibodies on their surface. Each cell has a specific single kind of antibody molecule, and now when I visualize a cell I see all the little thunderbirds sticking out, representing the antibody molecules crawling all over its surface. That visualization alone is tremendously valuable; it means that I can see all kinds of tricks. One of the tricks is to hook antigens to fibers, then throw the cells against the fibers to collect the specific ones and then pluck the fibers to release the pure antigen-binding cells containing a specific kind of antibody. Well, perhaps that's going to be fairly useful eventually in clinical medicine, for immunological engineering, for taking specific cells out of the body, for things akin perhaps to dialysis, and for probing certain diseases which have to do with deficiencies in different kinds of antigen-binding cells."

In the Footsteps of Einstein

A total eclipse of the sun shortly after the first World War enabled a group of English astronomers to confirm an aspect of Albert Einstein's General Theory of Relativity. Whereas the research of most classical physicists had led them to believe that the force of gravitation is exerted only by one mass of material upon another, that of Einstein indicated that it can be exerted also upon a ray of light. Pictures obtained during the solar eclipse by Sir Arthur Eddington and others demonstrated the correctness of Einstein's conclusion by showing that in the vicinity of the sun light waves bend.* At a dinner of the Royal Astronomical Society, Eddington underscored the significance of this revelation by reading a poem that parodied Edward FitzGerald's translation of *The Rubaiyat of Omar Khayyam* and concluded with these words:

---

* Edelman's reference, of course, is to Nobel Laureate James D. Watson, one of the scientists responsible for the elucidation of the structure of DNA, the cell material associated with the transmission of genetic information.

Oh leave the Wise our measures to collate.
One thing at least is certain, LIGHT has WEIGHT.
One thing is certain, and the rest debate—
Light rays, when near the sun, DO NOT GO STRAIGHT.[27]

The discovery that light waves are deflected when they enter the gravitational field of the sun took place in 1919. Forty years later, at the University of Maryland, Dr. Joseph Weber set out to test another aspect of the General Theory: Einstein's prediction of the existence of gravitational waves.

One of the implications of the General Theory is that when mass is destroyed some of the energy released—perhaps the bulk of it—takes the form of gravitational waves. As *Science*[28] magazine has noted, "gravitational waves are generated by accelerating masses, such as binary stars, and propagate with the speed of light." The magazine adds that when other "masses are crossed by the waves, they should in turn undergo alterations;" but as gravitational interaction is very small—about 10^{40} times smaller than that of electromagnetic forces, for instance—"detection of the waves is extremely difficult."

When in the late 1950's Weber began his studies, both the existence of such waves and the likelihood of their being observed, if they did exist, were subjects of controversy. As late as 1975 both matters were still in dispute. Since 1967 the detectors designed and built by Weber and his associates have repeatedly registered what Weber himself considers evidence of the arrival of gravitational waves from extraterrestrial sources, possibly from the center of our own galaxy, the Milky Way. Attempts by a number of scientists to confirm his findings, however, have so far produced no reports of success.

Does this mean that the hunt for gravitational waves is already a lost cause? Far from it. A few years back several scientists were on record as pessimistic about the outcome of Weber's experiment. Today the prevailing attitude is a more hopeful one. Writing in the January 1974 issue of *Physics Today,*[29] physicist Terrence J. Sejnowski of the University of California at Santa Barbara takes note of the failure of an international effort to duplicate Weber's results. "Nonetheless," he adds, "gravitational-wave detectors are improving dramatically and may soon be sensitive enough to detect gravitational radiation from known astrophysical sources. These advances are all based on Weber's original research on the theory of gravitational-wave detection and his construction of detectors to measure the dynamical curvature of space with unprecedented sensitivity." What we are dealing with here, in other words, is not as yet one of the success stories of modern science. It is the story of a boldly conceived experiment whose final chapter remains to be written.

---

* Those classical physicists who believed light to be composed of particles rather than waves did concede the possibility of its being subject to gravitational effects. Einstein's view of the matter was an outgrowth of his postulation that light exhibits the characteristics of both particles and waves.

By way of introduction, a few words about the central character: Joseph Weber is the son of a Lithuanian carpenter who came to this country with his wife in 1909 and settled in Paterson, N. J. There Joseph, the last of their children, was born 10 years later. He attributes his early interest in physics partly to a fondness for the subject on the part of an older brother, partly to "an outstandingly good physics teacher" in high school, Miss Hallie Turner, who had taken her master's degree at the University of Chicago under the great Albert A. Michelson.

When he finished high school in 1936, his father was out of work. "The other kids were supporting the family," he recalls, "and I couldn't expect them to send me to college." Happily for the young physics enthusiast, Senator William W. Barbour of New Jersey, instead of personally naming his candidates to the U.S. Naval Academy at Annapolis, had arranged for them to be selected by means of competitive Civil Service examinations. As a result Weber went to the Naval Academy, earning his first degree there in 1940. One is bound to wonder how the son of a Lithuanian-born carpenter fared in the not exactly unsnobbish social atmosphere of the Academy; but when that question is put to Weber, he delicately changes the subject. He saw service in World War II and stayed in the Navy until 1948. ("I figured I owed the Navy the time," he says.) He then joined the University of Maryland faculty, simultaneously enrolling as a student at Catholic University of America, where he received his Ph.D. in 1951.

His last job for the Navy was in the Bureau of Ships. It involved distributing some $50 million to industries doing business with the Government. Weber says that when he left the Bureau every industry to whom he had allotted money offered him a job. "To accept those positions," he says, "didn't seem quite proper, so I came to the University of Maryland, to whom I'd given no money." He's been there ever since, assuming his present post—that of professor of physics—in 1967.

He began thinking about looking for gravitational radiation in the mid-1950's. He was fully aware, as he later remarked, that it would be a "long-shot" experiment. The current thought was either that there were no such things or that the energy loss required to produce waves capable of being detected called for the destruction of so much matter—the equivalent of "a thousand suns a year—that some scientists didn't want to believe it was possible." One gets the impression from Weber's own recollections of the beginnings of his project that he went ahead with it, not in spite of these caveats, but because of them.

"The reason I decided to do it," he says, "is that I'd worked on the theory for a few years and while the theory was very difficult, the result seemed fairly clear, at least to me. With regard to the question of whether or not the waves existed, Einstein himself had changed his mind about that several times.

"In a book about him, Leopold Infeld* describes working with Einstein at Princeton** in the late 30's. By the time Infeld got to Princeton Einstein had decided that there should be no gravitational waves. He told Infeld as much, and as so often happens the younger man, Infeld, was carried away and proceeded to show his master an even more thorough way of proving that the waves didn't exist.

"Now in those days whenever Einstein announced that he was going to give a seminar, scientists couldn't get in the door because there were newspapermen and photographers all over the place. So, at Princeton, Einstein's seminars were not announced; it was just said that there would be a seminar in a certain room at a certain time and the scientists knew who it would be. Well, H. P. Robertson [well-known theoretical physicist] was at Princeton at the time and either Einstein or Infeld talked to him, and he looked over what they had done and found a mistake in it and showed them that it was incorrect, and that there really were waves. And so Einstein came to a seminar he had promised and reviewed what he'd done and said he'd been wrong and that there were waves. Infeld went right on thinking there weren't, but he was in a minority.

"Most people," Weber adds, "thought there were." In this connection he mentions the late J. Robert Oppenheimer, Freeman J. Dyson, John A. Wheeler and other eminent scientists who, from time to time, have made known their support of his efforts. In the late 1950's his own thinking was that "every generation should take a look at the unsolved problems from the standpoint of the present. Things which seemed impossible, say 30 years ago, might seem reasonable in the light of present technology. So I decided to give it a whirl."

---

* Weber's reference is to Infeld's *Quest: The Evolution of a Scientist* (New York, 1941).

** Einstein joined the Institute for Advanced Study in Princeton in 1933, working there until his retirement in 1945.

Once started, he quickly discovered why there had been no prior work. "The numbers one came out with were extremely discouraging," he says. "A textbook published in 1962 contains the statement that 'the weakness of the gravitational interaction makes it exceedingly unlikely that gravitational radiation will ever be the subject of direct observation.' Of course, one couldn't see a statement like that without feeling challenged."

Weber confesses that another facet of the problem also attracted him: Because little had been done in the field, little had been written. That meant "I didn't have to plow through a lot of literature to get started. I could just start."

He started in earnest in 1958. Since, as he puts it, "the numbers were sort of discouraging from the standpoint of making the waves in one corner of the room and detecting them in another, I thought, 'Well, there are big objects in astronomy. There are stars, galaxies. There's a lot more mass in the sky than you can get hold of on earth. Maybe some of those big objects might make the waves.' "

As no technology was available, his first step was to design an antenna capable of receiving the waves. He used Einstein's equations to accomplish this task. Theoretical physicists among his colleagues, when shown the finished design, pronounced it sound. "Build the antenna and look," they advised him. "If you don't find anything, you don't find anything."

And that, says Weber, was "the philosophy" on which he preceeded. The first of what have been several grants from the National Science Foundation—$36,000, in 1960—enabled him to hire a couple of assistants and to construct two antennas. He refers to these machines as "first generation" detectors for the reason that those built at later dates enjoy a number of substantial improvements. Suspended on acoustic filters in a vacuum chamber, each antenna consists of a large* aluminum cylinder instrumented to respond to frequencies believed to be associated with the propagation of gravitational waves.

To avoid false signals to the movement of the earth, Weber located one of his detectors at the Argonne National Laboratory near Chicago. The others operate in his own laboratory on the outskirts of the University of Maryland campus. For about 10 years data received at Argonne were transmitted to Maryland over a leased telephone line. Now more sophisticated mechanical devices take care of this function. Signals thought to be attributable to gravitational radiation are accepted as such only if they are registered by two or more of the detectors and only if they cannot be traced to other possible causes, such as seismic or electromagnetic effects. Those that fulfill these and other requirements are spoken of as "coincidences."

---

* The average cylinder is 153 centimeters long and 66 cm in diameter.

In 1967 Weber began getting coincident signals. Reporting on these findings in 1969, he cited the acquisition of a large number of two-detector, three-detector, four-detector, and even five-detector coincidences and described these events as implying the receipt of gravitational waves. So strong were the signals that it was considered likely that they were coming from pulsars (then only recently discovered) or from matter falling into a "black hole," the name given to a region of space into which a star or a collection of stars and other bodies has fallen and from which no light can escape.[30]

Weber told *Physics Today* that he had expected to find at most only two or three events a year. He was surprised, therefore, at the rapid rate—roughly one a week—at which the coincidences occurred. "It's hard to believe," he said, "that there won't be skeptics."[31]

There were. On the basis of admittedly limited data, a team of Russian researchers has suggested that Weber's coincident signals might be caused, not by gravitational radiation, but by pulsations of the magnetic field of the earth.[32] Perhaps the most frequent argument of critics is that so far as anyone knows there just isn't enough energy in the sky to generate waves strong enough to be acquired by Weber's cylinders. Weber meets this argument (as do some of his supporters, as well) with the observation that "only about 10 percent of the sky is visible to us" and there are indications that the remaining 90 percent contains quite enough energy to account for the detection of gravitational radiation by his antennas.

He reacts with some vigor to the reminder that a number of scientists, using detectors similar to his own, have so far failed to confirm his results.

"That statement," he says, "isn't entirely correct. One person has built apparatus, and there are coincidences between his apparatus and mine. However, he works for a commercial organization—a very well-known one—and he has requested that the information of his collaboration be kept secret. The National Science Foundation has been informed of the arrangement and has approved it. The idea of secrecy is not mine. It originated with his organization because it's part of the American establishment, and their concern is that I would use their name in vain by claiming that they're confirming our results before they themselves are sure that they have done so, which is perhaps understandable.

"With that one exception, no one else has confirmed the results. Still one might say that the situation on confirmation is not really quite as bad as one might think for a number of reasons. When we first saw the coincidences I read the charts alone and everyone had to believe what I said about them. Then I was replaced by magnetic tape and a computer. And the computer found the same coincidences. Now a report has been written by two other people—I'm not the author of it—who have found the coincidences in the data on the magnetic tape. This report has been submitted for publication."

What does Dr. Weber see beyond the point of confirmation? What developments should then be attempted in the field and what scientific and practical applications can the work be expected to yield?

"The next step beyond confirmation," he says, "is the improvement of the antenna; the development, if you like, of a kind of gravitational-wave telescope. The status of the machine we have now is about that of Galileo's astronomical telescope. It took several hundred years to move from that to the present 200-inch Palomar instrument and to radio telescopes just in terms of aperture, sensitivity, and things like that. The basic design of the antenna we now use has been accepted by everybody now. It is not controversial in any sense. Moreover the various steps needed to make it 10 billion times better are clear. It's not clear how long that will take or how much money, but it is clear that it's doable.

"Laboratory experiments were out of the question in 1959. If the right improvements are made in the instrument, then laboratory experiments will be possible. We'll probably see such experiments during my lifetime, done with machines 10 billion times better than the ones we now have.

"As for social benefits ultimately, it's really not out of the question to think in those terms because there's 100 times as much energy in the world in the form of gravity than in the form of nuclear fission and fusion. I think it would be wrong in any of your presentations to give the impression that the use of it is just around the corner. It's far more distant than, say, a plasma-physics fusion machine. The fact remains that most of the energy in the universe is in gravitational form. People like Freeman Dyson at Princeton have thought of exotic ways of getting the energy out—such as putting rocket engines on a star and then towing the star to the vicinity of another star and letting it fall in a certain way. Dyson remarks that it would take a very advanced civilization to do that sort of thing. An advanced civilization would willingly pay the tax money needed to get all that energy out of the stars for some noble purpose, such as making the deserts bloom in another galaxy. Finally and obviously, after confirmation we will have a new way of looking at the universe, a new window on it, over and above the ones that are now available to us."

One final question for the professor: Is he disturbed by the controversy his findings have evoked?

"I'm not losing any sleep over it," is the reply.

Actually Dr. Weber doesn't seem to get much sleep under any circumstances. Asked to comment on a book widely discussed in scientific circles, Thomas S. Kuhn's *The Structure of Scientific Revolutions*, he says he hasn't read it. He doesn't have time to read books these days. He spends 100 hours a week pursuing gravitational waves.

And that's how it goes—on what the late Vannevar Bush called "the endless frontier," the place where science moves ahead. Now for the organization that helps it move.

Chapter 1. Spotlight on Science

[1] Drake, *The Geological Revolution*, Condon Lectures, Oregon State System of Higher Education, Eugene, Oregon, 1970, Preface.

[2] Letter to Abbe Soulavie, read at meeting of the American Philosophical Society, November 21, 1788.

[3] Drake, ibid., p. 24.

[4] Except as otherwise indicated, all direct quotes in this chapter were obtained during interviews with the investigators mentioned.

[5] John F. Dewey, "Plate Tectonics," *Scientific American*, May 1972, p. 56.

[6] See *Continents Adrift: Readings from Scientific American*, with introductions by J. Tuzo Wilson, p. 39.

[7] Mauawai Barazongi and others, "Propagation of Seismic Waves Through and Beneath the Lithosphere that Descends Under the Tonga Island Arc," *Journal of Geophysical Research*, February 10, 1972, p. 952.

[8] Drake, "The Astounding Pulsars," *Science Year: The World Book Science Annual*, 1969, p. 37.

[9] Stephen P. Maran, "In nine centuries, search unravels many parts of the Crab Nebula mystery," *The Smithsonian*, June 1970, p. 56.

[10] Ibid., p. 42.

[11] J. P. Ostriker, "Nature of Pulsars," *Scientific American*, February 1971, p. 49.

[12] Anthony Hewish, "Pulsars," *Scientific American*, October 1968.

[13] Maran, ibid., p. 56.

[14] Ibid.

[15] Battelle Columbus Laboratories, *Interactions of Science and Technology in the Innovative Process: Some Case Studies*, Report prepared for NSF, March 19, 1973, p. 8-4 (cited hereafter as Interactions).

[16] *Washington Star-News*, October 18, 1974.

[17] *New York Times*, October 19, 1973.

[18] Ibid.

[19] See note 15. The Battelle report is the second such study to be funded by the Foundation. Its predecessor is the Illinois Institute of Technology Research Institute, *Technology in Retrospect and Critical Events in Science* (TRACES), vol. 1, December 15, 1968, and vol. 2, January 30, 1969.

[20] *The Structure of the American Economy, 1919-29*. Cambridge, Mass., 1941.

[21] Interactions, p. 8-8.

[22] *New York Times*, October 13, 1972.

[23] Jerne, "The Immune System," *Scientific American*, July 1973, pp. 52-53, 55.

[24] *New York Times*, ibid.

[25] Porter to Mrs. E. K. Engel, October 21, 1974.

[26] *New York Times*, ibid.

[27] Ronald W. Clark, *Einstein: The Life and Times*. New York and Cleveland, 1971, pp. 227-229.

[28] February 27, 1970, p. 1237.

[29] P. 40.

[30] Weber, "Evidence for Discovery of Gravitational Radiation," *Physical Review Letters*, June 16, 1969, pp. 1320-1324.

[31] "Weber Reports 1660-Hz Gravitational Waves from Outer Space," *Physics Today*, August 1969, p. 61.

[32] "Gravity Waves: Correlation with Geomagnetic Storms," *Science*, June 15, 1973, p. 1161.

Chapter 2

How Firm a Foundation?

The official life of the National Science Foundation began on May 10, 1950 when President Harry S. Truman signed a recently passed bill, S. 247, into Public Law 507 of the 81st Congress. Its active life began 7 months later, on Tuesday, December 12, when shortly before 10 a.m. a group of men and women began assembling in the Cabinet Room of the White House in Washington, D.C.

Chosen by the President and confirmed by the Senate, these people were the National Science Board. As such they comprised one-half of the new agency. At a later date Truman would add the other half by naming a Director who would then administer the Foundation and share its duties and powers with the Board.

Meanwhile the Board members were gathering to organize themselves, make plans and hear a talk by the President. Public Law 507 set their number at 24, but Charles E. Wilson, president of General Electric, had not yet got around to signing his commission of appointment, never would do so, would attend no meetings and within a few months would resign, to be replaced by another industrialist.

There was a drumbeat of social chatter as the members arrived, glad for the fire on the hearth under Seymour Thomas' portrait of Woodrow Wilson after the damp chill of one of Washington's gloomier days. There were some introductions. Not many, for although the members came from 16 States and the District of Columbia, most of them had run across one another at conferences or seminars connected with their mutual interest. Almost all of them were scientists or scientists turned administrators or industrialists or public affairs leaders whose work touched on science. One was a working engineer.

It was a gathering of talents, certainly. Recently appointed to the presidency of Johns Hopkins University in Baltimore, Md., Detlev W. Bronk, as the first investigator to record electrical activity in single nerve fibers, had opened to exploration an entirely new field of neurophysiology in the early 1920's. Gerty Therese Cori, an unassuming auburn-haired professor from the Medical School of Washington University in St. Louis, Mo., was a co-holder with her husband of the Nobel Prize for medicine and physiology. James B. Reyniers' experiments with animals in germ-free shelters at the

University of Notre Dame in Indiana had attracted worldwide attention. The studies of certain organic compounds by Robert P. Barnes of Washington's Howard University were appreciated wherever research in chemistry was of interest. Few American mathematicians were more respected by their peers than Professor Marston Morse of the Institute of Advanced Study at Princeton, N.J. Smiles appeared around the long mahogany conference table as one of several White House employees, coming and going in an adjoining corridor, was heard to call out, "Who are those people in the Cabinet Room this morning?" and another to answer, "Oh, nobody really. Just a bunch of high brows!"[1]

New as it was, the organization these accomplished individuals had been summoned to induct already had a long and turbulent history. During the preceding 5 years, no less than 21 National Science Foundation bills had been introduced and heatedly discussed in Congress and in the press. When President Truman finally put his signature on one of them, the magazine *Industrial and Engineering Chemistry* pondered editorially, "How Firm a Foundation?"[2]

None of the reservations implicit in editor Walter J. Murphy's question dealt with the form of the new agency or with its purposes. If Murphy sensed hazards in the offing because the Foundation was "a two-headed monster," its authority about evenly divided between Board and Director, he failed to mention them. Nor had he fault to find with what he took to be the agency's "principal objective." This he described as "the promotion and support of basic scientific research" by means of "contracts and grants to schools and other organizations." He was gratified to discover in the NSF enabling act evidence that its unscientific Congressional framers defined basic science much as he did. Murphy called it "the search for knowledge where no practical application for that knowledge is evident."

He looked upon all of these elements as good, but in the charter of the NSF he discerned at least three potential problems. These he identified as "the scope of the subject matter to be covered, evaluation of the research projects after they are completed, and obtaining and maintaining a stable financial basis."

What most worried editor Murphy was that the same clause in the law that directed the Foundation "to initiate and support" basic science also obliged it to appraise the impact of such research "upon industrial development and upon the public welfare."

Did this provision mean that the new agency would have to justify its programs by pointing to practical effects? If so, it was in for trouble. How generous would future Congresses be as it became apparent that the precise outcome of fundamental scientific exploration cannot be foreseen?

"Many of the very real links between basic research and the general welfare," Murphy wrote, "are too subtle and interconnected to be traced by human perception. We know they are there, but our knowledge contains a strong element of faith that goes beyond what can be measured and thereby proved."

It is interesting in retrospect to note that in 1950 neither Murphy's magazine nor apparently any other scientific publication pointed out another possible source of difficulty in the NSF enabling act. The act bade the new agency "to develop" a national science policy and to "evaluate" the research programs of other Federal agencies. In the years to come these instructions would try the patience and test the vision and skill of the Foundation's managers.

Without doubt the members of the National Science Board were aware of the pitfalls ahead as they began their deliberations in the Cabinet Room of the White House on that gray winter morning in 1950. Still, the prevailing atmosphere was one of cautious optimism.

To Lee A. DuBridge, President of California Institute of Technology at Pasadena, the occasion marked "the beginning of a New Deal for American science." To Professor Morse it was "a pleasure to reflect that after years of argument in and out of Congress," the National Science Foundation was at last actually in existence.

"At that organizing meeting," he would recall later, "we went about our business calmly enough, even sedately, but one sensed a strong undercurrent of excitement and anticipation."

Whatever the feelings in the Cabinet Room, they seem not to have spilled into the world beyond. There was no dearth of news in the press that day, most of it unpleasant. Twenty-five thousand American marines were fleeing across the "frozen hell" of Korea, pursued by a superior Chinese force. One Congressional committee was investigating racketeering in New Jersey. Another was listening to a witness bent on showing that a high Government official, the American Broadcasting Company, and the Metropolitan Opera were jointly involved in "a Communist conspiracy." The Harvard student magazine, *Lampoon*, was protesting a $100 fine for publishing obscenity, and in grocery stores up and down the Atlantic Coast eggs were selling for a dollar a dozen.[3]

Understandably, perhaps, the press had little space for the activities of a small and untried Federal bureau. Of the four daily newspapers then in Washington, only one mentioned the meeting in the Cabinet Room, the *Washington Post*, giving it a line of type in its daily feature, "The President's Calling List." Even the verbose *New York Times* found five short paragraphs on an inside page sufficient for recording the event.[4]

It was a quiet beginning for an organization that during the next quarter of a century would leave an identifiable impression on many aspects of American life.

Chapter 2. How Firm a Foundation?

[1] This chapter rests largely on NSB Min 1: 1-2, supplemented by interviews with Board members Sophie B. Aberle, Detlev W. Bronk, John W. Davis, Lee A. DuBridge and Marston Morse.

[2] July 1950, p. 1265.

[3] *Washington Post*, Dec. 11, 12, 1950; Washington *Evening Star*, same dates.

[4] *Post*, Dec. 13, 1950; *New York Times* (p. 15), Dec. 13, 1950.

Chapter 3

Old Fears and New Ideas

The establishment of the National Science Foundation in 1950 involved questions as old as the Nation: Was a partnership between the Federal Government and the American scientific community feasible? Could such a partnership be shaped along lines compatible with the Government's fiscal accountability to the people without diminishing the freedom indispensable to scientific inquiry?

The United States Constitution as ratified into life in 1789 did not in so many words permit Congress to finance scientific activity. From the days of Thomas Jefferson's presidency on, however, the legislators found ample sanction for doing so in those clauses of the basic law that gave Congress the power of taxing to promote the general welfare, defend the country and regulate commerce.

The country's dependence on science was recognized from the beginning. Shippers and other commercial interests needed information about the coasts and harbors along the Atlantic and only the central government could assemble and fund the requisite scientific surveys. Prodded by the spirit later called "manifest destiny," the country expanded rapidly. The Louisiana Purchase added millions of acres to the public domain. The war with Mexico added millions more. To supervise the settlement of these vast reaches the Government had to know which lands were fit for farming, which for mining. Where could wagon roads be run and railroad tracks laid? What were the flora and fauna? These questions could be answered by expeditionary forces organized by the Federal authorities and staffed with scientists trained in a variety of disciplines.

Early nineteenth-century Congresses moved slowly in such matters. Their practice for over 60 years was to purchase whatever research the country needed on a strictly *ad hoc* basis. Consistently they avoided the long term commitments of money that might eventuate in the creation of a Federal science establishment.

Their caution was natural to men still profoundly influenced by the national attitudes born in the fires of the American Revolution. A people who had rebelled against a powerful central government in England were understandably chary of adding functions to the powerful central government that circumstances had compelled them rather against their will to set up on their own soil. It is illuminating, in this connection, to recall that America's earlier experiment in national rule—the ill-fated Confederation—was too decentralized to work; and that the Federal Convention of 1787 still remains history's most enlightening example of thoughtful men painfully hammering into existence a central government at once strong enough to meet the Nation's needs and centrifugal enough to preserve the integrity of the State governments and the liberties of the citizenry.

It was only after the principle of State sovereignty was sharply downgraded by another revolution, the Civil War, that permanent scientific bureaus began to materialize within the Federal framework. To be sure, the Coast Survey, now the Coast and Geodetic Survey, has enjoyed a continuous existence since its founding in 1807. But of the status of that Federal agency prior to the Civil War, A. Hunter Dupree writes: "even though its job lay swelling in front of it to give an illusion of permanency, the Survey remained in theory an *ad hoc* enterprise. If the day ever came when Congress considered that the coasts had been adequately mapped, the Survey's reason for existence would vanish." Speaking of the period immediately after the Civil War, however, Dupree notes that "in the two generations following 1865 a great scientific establishment arose within the government, developing its own typical organization around the problems it faced, recruiting a specialized personnel, and contributing greatly to the scientific resources of the country."[1]

During this same period the Government began reaching out to the universities, albeit on a limited scale. By the end of the nineteenth century agricultural science was receiving considerable Federal support at a number of such institutions. Two decades later the National Advisory Committee for Aeronautics, created in 1915, was using contracts for awarding money to university scientists, for basic research in the committee's field of interest.[2]

These developments, however, were not tantamount to the creation of a partnership between the national government and private science. The organic act behind every scientific bureau was imbued with the understanding that the agency would concentrate on "practical" science—on what is now spoken of as applied science, meaning the search for practical uses for the concepts and data that pure or basic science produces. The risky penetration of the unknown was regarded as belonging to the private sector—to the universities and other non-government centers of research. The line between the two sectors was not to be crossed, not at any rate by public dollars. There was no animosity toward basic research per se in all this; only the determination of early 20th century Congresses, and of the Nation's scientific leaders, to maintain a wall of separation between the Federal Government and private science—a wall that remained substantially unbreached until with the coming of the Great Depression in the early 1930's small cracks began to appear in it, portents of a new dispensation in the making.

Needing the guidance of science in meeting the problems of that troubled period, President Franklin D. Roosevelt arranged for the installation within the National Academy of Sciences[3] of a council of experts headed by Karl T. Compton, the gifted and personable president of Massachusetts Institute of Technology in Cambridge. Known as the Science Advisory Board, this short-lived group began at once diffusing a philosophy at odds with the one in vogue.

An important lesson of history, as Compton and his fellow board members read it, was that there was a limit to what applied science could accomplish unless it was continuously nourished by the discoveries of basic science. Since its beginnings the United States has relied for most of its fundamental scientific data on European scholarship. With the country becoming increasingly dependent on technology, the time was fast nearing when such imports would not suffice. Against that day, the Advisory Board proposed that the Federal Government strengthen America's own basic science quotient, not by infusing more of such work into the Federal agencies, but by grants-in-aid to scientists already pursuing original research in the laboratories of academe and industry.

There may have been some enthusiasm for this idea among industrial scientists, but there was little or none among the academic ones, most of whom were fearful that Federal assistance on a large scale would pave the way to Federal control of their institutions.[4] Nothing came of the Science Advisory Board's suggestion in the early 1930's. The concept behind it, however—the view of basic science as a national resource necessary to national survival and, therefore, worthy of public support—this idea was soon to have its day.

Even as Karl Compton's Advisory Board was proposing and his colleagues around the country were disposing of the most ambitious attempt to date to forge a link between Federal Government and private science, events on the far side of the Atlantic were preparing the cataclysm which would make such an arrangement appear not only desirable but overdue.

The outbreak in 1939 of the second World War plunged the United States into a frenzy of military preparation against the eventuality of direct American involvement—a step dictated 3 years later by the disastrous Japanese attack on Pearl Harbor.

Prominent among the American scientists who were watching these developments with concern at least as early as 1939 was the 47-year-old President of the Carnegie Institution of Washington, Dr.

Vannevar Bush.* Tough, brilliant, versatile, abrasive on occasion, Bush was an electrical engineer and physicist by training—and interested in much else besides. *Science is Not Enough,* he titled one of his published collections of essays, and he explored many things and wrote about many things. One of his interests was management. He called it an "art." The techniques of it could be taught, he believed, but the art must be learned. One was either born with the art or one achieved it through a passion to excel and a commitment to ideals. By one route or the other or both Bush had most certainly achieved it. He was a consummate administrator. Before World War II he was best known as the inventor of an ingenious calculating device, the differential analyzer. After the war his good friend, Harvard President James B. Conant, would assert that Vannevar Bush's "greatest invention" was the Office of Scientific Research and Development,[5] the remarkable emergency agency through which the scientists of America helped win the great conflict by supplying the military with weapons and other instrumentalities of war unknown, and for the most part undreamed of, when the conflict began.

The story of OSRD has been told in detail elsewhere. Here only two aspects of it need detain us. One is the unique nature of its methods, without which a viable Government-science relationship might have remained indefinitely unattainable. The other is its significance as a forerunner of the National Science Foundation.

Soon after leaving the vice presidency of MIT to become president of the Carnegie Institution in 1938, Bush had become first a member and then chairman of the National Advisory Committee for Aeronautics, the 12-member agency established by Congress in 1915 to "supervise and direct the scientific study of the problems of flight." It was while watching events in central Europe from the vantage point of NACA that Bush became aware that his country was already "living in a prewar, not in a postwar period."[6] Plainly a confrontation of armageddon proportions was in train, one that would test the fiber of free institutions and whose outcome would depend less on the size of democracy's batallions than on the ideas generated by its technologically trained minds. Obviously science must play an unprecedentedly large role in this conflict. But by what means, and through what sort of organization?

Early in 1939 Bush began discussing these questions with scientific colleagues, among them Compton of MIT and Conant of Harvard. Back in 1918 American scientists had gone to war, but they had done so in uniform. As soldiers they had worked as a rule only on problems submitted to them by the Army and Navy. In view of the enormous quantities of research World War II would absorb, the consensus of Bush and his associates was that this time a different policy must rule. In this conflict the scientists should be free to initiate problems. The arrangement, in other words, should be a two-way street, with the Military assigning problems to science and science in turn suggesting projects to the Military.

---

* Dr. Bush died on June 29, 1974. He was 84.

A few years after the second World War the importance of this decision would be underscored by Don K. Price, Jr., onetime staff member of various Federal agencies close to the world of science, now Dean of Harvard's Graduate School of Public Administration. At an earlier period, Price would point out in a perceptive study of the Government-science kinship, "our military sources thought that civilian scientists in military laboratories should conduct their research only pursuant to 'requirements' defined by military staff work." In the second World War, Price recalled, this "notion was exploded as it became apparent that what scientists discovered by unrestricted research might be of greater military importance than the things the military officers thought they wanted—in short, that the means might determine the ends."[7]

As for the sort of scientific organization appropriate to World War II—arriving at an answer to this question was largely a process of elimination. There were already in existence at least two Government or Government-connected bodies that might do. One was NACA. The other was the National Academy of Sciences, chartered by Congress during the Civil War to supply executive agencies with technical advice and strengthened during World War I by the addition of its working arm, the National Research Council. But NACA was concerned solely with aeronautics, and the Academy and its Council were quasi-private advisory bodies. To make something else of either would be like trying to render an old building suitable to new functions by tacking on some sheds and outhouses. Better by far, Bush and his associates concluded, a new building, constructed from basement up to take care of the needs at hand.

During the summer of 1940 Bush was able to place the problem before President Roosevelt at a White House conference. Bush's later recollections of how this meeting came about are these:

"My work with NACA had brought me into considerable contact with Harry Hopkins, the President's close friend and assistant. It was with Hopkins that I first discussed the notion of a wartime science agency. He in turn discussed it with the President, and one day—in early June it was—Hopkins took me to see Mr. Roosevelt. The conference in the executive office, at which the agency was born, took all of 5 minutes."[8]

In his autobiography, Conant reconstructs with relish his reactions to a phone call from Bush on the 14th of that month.

"The idea," Bush told the President of Harvard, "has now been given form." There was to be a new organization with Bush in charge and Conant as one of the members. There was to be a two-way arrangement under which the participating scientists would continue to function as civilians. Conant could barely contain his joy: He decided that what he was hearing "marked the beginning of a revolution."[9]

The organization that started in June 1940 was not called the Office of Scientific Research and Development. It was the National Defense Research Committee. But NDRC had been operating officially for only a year and a day when it was broadened into OSRD, chiefly to permit the organization to fund and administer the medical research that the war effort required.

As officially constituted on June 28, 1941, the Office of Scientific Research and Development embraced two major wings—the NDRC, to support and supervise the general run of research and development projects; and a Committee on Medical Research. At this point Bush moved up to directorship of the entire organization with Conant replacing him as chairman of NDRC and Dr. A. N. Richards, a pharmacologist with the University of Pennsylvania, at the helm of the medical branch.

For projects requiring a variety of fixed facilities OSRD financed a laboratory at a suitable university and the civilian scientists hied themselves hence. Thousands of them simply went on working at their own academic or industrial installations. Out of their efforts came the proximity fuse, the crucial early explorations leading to the production under Army aegis of the atomic bomb, dozens of new medical procedures, enormous advances in the detection of enemy ships and planes by radar—to mention a few items on a long list.

As Irvin Stewart makes clear in his administrative history of the agency,[10] however, OSRD's contribution to today's Government-science partnership did not lie in what the organization did. It lay rather in the methods that Bush and his staff contrived for doing it.

OSRD ran no laboratories and performed no experiments. It achieved its goals by means of a contract, drawn up usually between the agency and a non-government institution. Convinced by an early study that scientists could not work effectively under the procurement contracts used by the Army and Navy, the staff of the agency formulated a document tailored to its own novel way of doing things.

At the heart of this document were two clauses, reading, "Whereas, the Government desires that the Contractor conduct studies and experimental investigations as hereinafter specified . . .; and Whereas, the Contractor is willing to conduct such studies and experimental investigations on an 'actual cost' basis," etc. Nearly all of the 2,260 contracts entered into involved two members of OSRD in the transaction. One was called the "Contracting Officer." His duty was to oversee business and fiscal matters. The other, the "Scientific Officer," administered the scientific aspects. Every contract called for the presence at the contracting institution of people "qualified" to run the business phase of the project at that end and to do the research and development requested. An "actual cost" provision—the existence of competency and responsibility on both sides—these were the significant elements of a contract that Dr. Stewart described as combining "a maximum freedom for the exercise of scientific imagination . . . with those safeguards necessary for the expenditure of public funds."

One of the agency's earliest decisions was to eschew publicity for itself. Credit, insofar as security considerations allowed credit to be given, went to the Military. OSRD had no public relations department. But if during the war years the public heard little about OSRD, the American scientific community and key figures in Washington's political circles occupied ringside seats on its operations. On many of these close observers OSRD's deeds and methods produced a profound effect. The scientific press began to reflect a change of heart among many of its readers—a growing sense that a financial link with the Federal Government would not necessarily put them on the first stage of the road to the hell of Federal regimentation.

Roosevelt took an active and personal interest in the agency's activities and problems. Indeed Vannevar Bush's easy access to the presidential ear was one of OSRD's greatest assets—one that some future National Science Foundation Directors would remember with a certain wistfulness. Congressional leaders took note of the country's increasing indebtedness to its scientists. And both in and out of the government some observers began thinking beyond the present emergency to the day when the war stopped and OSRD disbanded, leaving a vacuum in the affairs of the Nation that in some manner would have to be filled.

It is impossible to say just when or in whose minds the notion first took root that a peacetime successor to OSRD should be constructed. Reading the old documents and the wartime press, one gets the feeling that it was just one of those ideas which finds its way into the air because its time has come. All that can be said with certainty is that the first movements in this direction were largely the work of Bush and his close associates on the scientific side, and of Democratic Senator Harley M. Kilgore of West Virginia and his staff on the political side.

When he came to the Senate in 1940 Harley Kilgore was a 47-year-old lawyer and criminal court judge from a small coal-mining town in the West Virginia hills. He had a mechanical bent, had studied engineering in his younger days, and corresponded regularly with

Perry Daniel Strausbaugh, professor of botany at West Virginia University in Morgantown. In short, the Senator brought to his legislative duties a scientifically oriented mind. On becoming a member of the Military Affairs Committee, he was quick to perceive the importance of the country's technical resources to the prosecution of the war. It was partly because of his background, partly at the urging of his research assistant, Herbert Schimmel (who thought of science as an instrument of social change) that in 1942 and again in 1943 Kilgore introduced bills aimed at establishing an Office of Scientific and Technological Mobilization to conduct and finance scientific and technical work, coordinate all Federal and private science, engage in international projects and promote the training of student scientists. Neither of Kilgore's technological mobilization bills went beyond the legislative committee to which it was referred. Both, however, were the subject of extensive hearings in the Senate. Neither was geared primarily to peacetime conditions, although the 1943 bill—S. 702 of the 78th Congress—described one of the purposes of the proposed agency as the facilitation after the war of "the transition of the national economy . . . to peacetime enterprise."

The first Kilgore bill drew only passing attention. Not so the second. Within 6 months after the introduction of S. 702 the Senator had received over a thousand letters about it, and a vigorous discussion of its merits was under way in the press.

Scientific reaction tended to be negative. Asked by Kilgore to outline his objections to the measure, Vannevar Bush did so in a 12-page letter. The OSRD Director's principal complaint was that S. 702 might give permanence to restraints that for patriotic reasons American scientists were willing to endure during the war but that they would not and should not accept after it was over.

The OSRD Director reminded the Senator that science had more to offer the American people than material comforts annd protection from their country's enemies. Its "broad object," he wrote, "is to increase the knowledge and the understanding of man. It aims at extending his grasp of the environment in which he lives and his appreciation of the vast and intricate system of nature by which he is surrounded." Bush added that as "science flourishes to the greatest degree when it is most free, I feel strongly that when peace comes we should bend our efforts . . . to return the maximum of independence to our scientific institutions and our scientific men."[11]

Kilgore may have winced at the lofty tone of this advisement but he took it seriously. He also examined the criticisms of his proposal raised by the Academy and other scientific organizations. Early in 1944 he asked his staff to revise S. 702 with the idea of producing a measure more palatable to the scientific community. The result was the issuance in late August of a draft document that contemplated the installation of an "Office of Science and Technology" specifically designed for peacetime ends.[12] Close in spirit and detail to the National Science Foundation bill that the Senator would introduce a year later, this document (technically a Confidential Committee Print) was submitted for comment to OSRD.

There were no official reactions from that quarter. "The reason there weren't," Bush would reveal later "was that I didn't like any of the Kilgore bills and had the feeling that the Senator didn't really understand what was in them. He had turned the writing of them over to his staff. Consequently the bills largely reflected the ideas of his research assistant, Herbert Schimmel. Previous experience had convinced me that trying to get Schimmel to change course would be a waste of time."[13]

So matters stood when in mid-October 1944 Harry Hopkins and Oscar S. Cox, general counsel for the Foreign Economic Administration, met to discuss an idea that seems to have originated with Cox. As head lawyer for the Lend-Lease Administration from 1941 to 1943, Cox had watched with interest the contributions of OSRD to the country's military effort. Would it not be a good idea, he ventured, to prevail on President Roosevelt to send a letter to Bush, asking the OSRD chief to suggest means by which the methods and findings of OSRD could be used in the interests of industry and the public welfare after the war?

According to historian Daniel J. Kevles of the California Institute of Technology, it seems that Cox at this time did not have in mind the creation of a government agency for the support of fundamental research. Basing his conclusion on a study of Cox's diary and other contemporary documents, Kevles writes that all Cox wanted was "an assessment of how the technical results of defense research, the inventory of new technologies and new knowledge, could be used to create a higher standard of living and full employment in peacetime." Kevles' impression is that Cox was thinking not only of science, but also of politics; that he saw in the proposed letter from the President to Bush a piece of ammunition that the Roosevelt forces could use on the eve of the presidential election in November.

If such were Cox's motives, he failed to reckon with the often amazing ability of scientists to turn political ploys to their own advantage. Having drafted the sort of letter he thought the President should sign, Cox showed it to Bush and Oscar M. Ruebhausen, the general counsel for OSRD. At this conference on the afternoon of October 24, the discussion quickly went to the latest Kilgore bill. Bush voiced his dislike of the West Virginia Senator's proposals, but said he would favor the establishment after the war of some sort of governmental mechanism for the support of scientific research in the universities. Cox offered no objections to this intrusion of a new dimension onto his ideas; and under the assumption that no legislative action on the Kilgore bill would occur until the new Congress convened in January, he and Bush agreed that during the interval they should try to develop a new legislative approach to the whole problem.

Immediately after this meeting Ruebhausen drafted for the President's signature a new letter, incorporating Cox's original ideas along with those of Bush and Conant. On October 27 Ruebhausen sent it to Cox. In turn Cox passed it on to Harry Hopkins with a covering memorandum in which he reported Bush and Conant as suggesting that release of the President's letter be delayed until after election day (November 7) on the grounds that its publication beforehand might prejudice the objectives they hoped it would accomplish.

So matters worked out. Hopkins held the letter on his desk until the day after the election when, having been reminded of its existence by Cox, he scrutinized it carefully and suggested some changes. In his original draft Ruebhausen had taken note of the high "national toll from heart disease and cancer. . . ." Hopkins' objection to this was that "everybody has to die of something. . . ." The draft also asserted that the Government "should assist the research laboratories of both the universities and industry in financing basic scientific research. . . ." Hopkins thought this statement too emphatic, and requested that it be mentioned simply as a possibility. He also considered the letter overlong, and asked that it be kept to two pages.

Cox rewrote the draft accordingly, and to continue with Professor Kevles' account: "While some of Ruebhausen's prose was excised at this point, there was no major departure in substance from the original draft. Cox sent the new version back to Hopkins, who endorsed it enthusiastically over to Samuel I. Rosenman, FDR's leading wordsmith and adviser. Conferring with Bush and Conant, Rosenman spruced up the letter, making the language crisper and more in line with FDR's style, and got the President to sign it. . . ."[14]

Dated November 17, 1944, Roosevelt's letter expressed the hope that "in the days of peace ahead" ways could be found for using the science-support techniques OSRD had devised during the war "for the improvement of the national health, the creation of new enterprises bringing new jobs, and the betterment of the national standard of living." How, the President asked, should the government go about abetting "the research activities of public and private organizations?" What policies should regulate the relations between these two groups? The failure of the World War II Selective Service Act to exempt students of science from the draft had created a dangerous deficit in the country's scientific manpower. Could a program be devised for easing this problem by "discovering and developing scientific talent in American youth?" The President wanted Bush's "considered judgments" on the points raised by these and related questions.

Roosevelt would not hear the answers. He was dead before Bush, assisted by four committees of scholars, could complete his now famous report, *Science, the Endless Frontier.*[15] As submitted to President Truman on July 5, 1945, the report was a persuasive presentation of the case for expanding the role of the Federal Government as a patron of science, and especially of fundamental science.

"Basic research leads to new knowledge," Bush wrote. "It provides scientific capital. It creates the fund from which practical applications of knowledge must be drawn." World War II had drastically reduced the capacity of European laboratories to produce fundamental data for export. Henceforth America must rely on its own resources. "A nation which depends upon others for its new basic scientific knowledge," the OSRD Director warned, "will be slow in its industrial progress and weak in its competitive position in world trade, regardless of its mechanical skill."

The report's chief recommendation was that a focus be given the Government's new scientific role by creating what Bush and his aides first spoke of as a National Research Foundation, later as a National Science Foundation.

Truman released the report to the public on July 19, and on that same day, a Thursday, Democratic Senator Warren G. Magnuson of Washington introduced S. 1285 of the 79th Congress, a bill that the OSRD staff had drafted and that incorporated Bush's preferences concerning the form and scope of the projected Foundation.

Kilgore was taken by surprise. His staff had further refined the West Virginia Senator's bill, and right up to the introduction of the Magnuson bill on July 19, Kilgore had counted on the cooperation of Dr. Bush. His annoyance was in no way lessened by the discovery that Bush was out of town that Thursday and unavailable "even by long distance telephone."

According to a memorandum circulating at the Bureau of the Budget, "Senator Magnuson had spoken to him [Kilgore] 2 days earlier about the possibility of introducing a bill on the subject of scholarships and fellowships only." The West Virginian had offered no demur to this idea; his own bill limited assistance to science students to their first 5 years in college, but he had let it be known that he would support a more extended program were it added to his proposal by amendment. The Budget Bureau memo pictured the West Virginian "as mad as everything" and convinced that he had been "doublecrossed."[16]

On the following Monday the disgruntled Senator introduced his own bill (S. 1297) and almost at once the marked differences between the Kilgore and Magnuson proposals became the fulcrum of a controversy that would hold off activation of the National Science Foundation for nearly 5 years. During this protracted debate, to be sure, many of the issues raised by the first bills were overtaken by events and winnowed out.

The question of patents, for example: Kilgore's bill stipulated that all inventions and discoveries resulting from research financed in whole or in part by the Foundation were to become the property of the United States. Magnuson's bill left the Foundation free to negotiate patent arrangements from case to case under guidelines enjoined by the public interest.

In 1945 this seemed an important matter because everybody assumed that the Foundation would sponsor a good deal of applied research for the Military and in the field of medicine. But this was not to be, owing to the alterations in the total Government-science structure that accompanied its conversion from war to peace. One of these was the establishment in 1946 of the Office of Naval Research, followed by a strengthening of the science components in other parts of the Service—changes that diminished the likelihood of the Foundation's dealing in any large way with military research. Simultaneously Congress began pouring more and more money into the Public Health Service's National Institutes of Health. These burgeoning appropriations strongly indicated that the Foundation, when it appeared, would handle only a limited quota of medical research, hardly any of it on the applied science level.

As little by little it became evident that the Foundation would concern itself chiefly with basic science, an area unlikely to yield patentable discoveries, the patent problem became academic.

The most durable issue invoked by the differences between the Kilgore and Magnuson bills was the question of who was going to boss the Foundation. The Kilgore bill vested the powers of the agency in a presidentially appointed single administrator assisted by a purely advisory Board. Under the Magnuson bill the powers would reside in a presidentially appointed Board whose members would choose an administrator to run the agency under their direction. Kilgore's bill, in short, made the Foundation directly responsible to the President. Magnuson's bill placed it under the control of the scientists.

The argument over the two bills gathered momentum in an atmosphere charged with the problems issuing from the stunning events of August 1945: the devastation of Hiroshima by the first atomic bomb, of Nagasaki by the second, the surrender of Japan, the ending of the war, the beginning of a troubled peace. Well before summer's end it was understood in both the executive and legislative branches of the Government that the time had come to transfer control of the terrible new force responsible for these events from military to civilian hands. Ahead lay the torturous Congressional battle that a year later would produce the Atomic Energy Commission.

As this development began Vannevar Bush was troubled by the possibility that, having created AEC, Congress would consider its scientific chores discharged and forget all about the National Science Foundation. The door to the White House no longer stood open to Bush as it had back in the Roosevelt days. He was compelled to convey his worries to Truman by means of an intermediary.

In early August he wrote his friend George L. Harrison at the Pentagon that it was imperative that Truman ask Congress to set up both the AEC and the NSF. Bush's argument was that AEC, as then planned, was "adapted for control and not for the conduct of research." He was certain that "any attempt on its part to enter this field administratively would be disastrous." Bush's fear was that were Truman to send Atomic Energy Commission legislation to Congress without also mentioning the Foundation, the ensuing discussion there would be marked "by confusion by reason of lack of guidance." He did not consider it necessary for the President to mention "both angles" in one message. Two messages "sent simultaneously or spaced in time by a brief interval" would be fine. He hoped that Harrison would so inform the White House.[17]

Evidently someone did so. In the fall Truman sent two special science messages to Congress. One called "for the establishment of a single Federal research agency"—NSF. The other asked the legislators to set up the Atomic Energy Commission as speedily as possible.

By this time the lines of battle were forming around the differing organizational structures proposed for the National Science Foundation in the Kilgore and Magnuson bills. Truman was known to favor the "in-line" organization set forth by Kilgore and within the scientific community a division was taking shape—a seemingly uneven one—with most of the more vocal scientific leaders sharing the view of Vannevar Bush that no Foundation at all might be preferable to one subject to the political controls that the OSRD Director read into the Kilgore bill.

On Capitol Hill other bills surfaced, calling for different kinds of scientific agencies. All destined to die in committee, these efforts would prompt Senator Magnuson to remark that "after Kilgore and I put in our bills, everybody got on the bandwagon."[18] Although Kilgore was still visibly miffed at both Magnuson and Bush, his heart was set on seeing the National Science Foundation in operation. It was largely at his instigation that in the late fall he and Magnuson together opened a series of hearings aimed at obtaining the guidance they would need to fashion a compromise bill tolerable to the scientists and acceptable at the White House.

Later on there would be other Congressional hearings on the subject but those that began in the Senate on October 8, 1945 and continued for two weeks were the most comprehensive and instructive. Ninety-nine witnesses appeared: scientists, Government officials, industrialists, labor leaders, men prominent in public affairs. These individuals and others (who submitted their ideas in writing) spread 1,196 pages of testimony on the records—a textbook, really, on the problems to be solved, the pitfalls to be avoided and the nuances to be weighed in arriving at workable science legislation.

Easily the most striking feature of the hearings was the statement from witness after witness that the country needed the proposed Foundation. Only one said otherwise.

The lone dissenter, Frank B. Jewett, stood high in scientific circles: President of the National Academy of Sciences, Director of Bell Telephone Laboratories, one of the architects of OSRD. A faint nostalgia wafted from Dr. Jewett's remarks. He wanted scientists to continue as in the past to get the bulk of their financial help from private enterprise and private foundations. He realized that under the tax laws of recent decades, the accumulation of the great fortunes necessary to create big private foundations had become impossible. His advice to Congress was that instead of erecting another Federal bureau, it change the tax laws so as to encourage greater support of science from private sources.

Jewett's testimony was the voice of the 19th century talking to the 20th, and probably the splendid old electrical engineer was as aware as anyone present of how deaf every century is to its predecessor. In 1945 only 4 years of life remained to Jewett. He would persevere in his crusade to save his fellow scientists from what he regarded as the imprisoning embrace of Federal largesse. A year later, in a futile effort to head off the strong operating agency most of his colleagues desired, he would persuade friends in the Senate to introduce a bill authorizing the President to set up a self-perpetuating corps of 50 "eminent scientists, educators and public affairs leaders" whose duties would be limited to telling Congress what the colleges and universities needed in the way of scientific research and training and to dispense whatever sums Congress made available for these purposes.

Among the many witnesses favorable to the Bush-Magnuson way of running the Foundation was President Conant of Harvard. The brilliant chemist-educator was "not one of those" who felt "that everything the Federal Government touches is bound to be so political as to be . . . inefficient." On the contrary he believed that "if properly organized" the Federal Government could do the job as well as a private foundation. But he emphasized the phrase, "if properly organized." To Conant it was obvious that:

> A Board will be more likely to take an impartial view than a single administrator, less likely to be subject to the harassing pressure of constituents who quite properly turn to their elected representatives every day in Washington. I am very much afraid that the scheme will be a failure if a single administrator is provided; with a board we stand a much better chance. . . . A board, unlike an individual, is hard to get at and harder still to push around. . . .[19]

The most throughgoing rationale for the single-administrator organization envisaged by Senator Kilgore came from Harold D. Smith, Director of the Bureau of the Budget. Speaking to the scientists in the manner of a father outlining the facts of life to a son on the threshold of adulthood, Smith said that in assuming that the board organization was better, they were confusing appearances with reality. The board idea looked good, but in actual practice a group of scientists unfamiliar with the curious ways of Washington would be singularly vulnerable to political inflitration. On the other hand, to put the Foundation under a single administrator close to the President would be to shield it from undesirable pressures by assuring it the protection of the most powerful office in the land.

Into a flow of testimony largely complimentary to science, former Congressman Maury Maverick injected a risible dissent. Speaking as the General Manager of the Smaller War Plants Corporation, the colorful Texas Democrat took issue with much that was being said and particularly with the remarks of Isaiah Bowman, president of Johns Hopkins University.

In directing his barbs at Dr. Bowman, Maverick took on an impressive adversary. With his squared-off Wilsonian face, his penetrating eyes behind steel-rimmed glasses, Isaiah Bowman cut a formidable figure. He had taught geography in his younger days and nothing irked him more than to hear it gossiped around campus that his subject was a snap. It was no snap in Professor Bowman's classroom. Even after his noteworthy abilities carried him to leadership of the National Geographic Society, to the post of chief American territorial specialist at the Versailles Peace Conference following World War I, and finally to the administration of Johns Hopkins, Bowman (as a colleague remarked) remained "the incorrigible teacher."[20]

Leading off the proceedings as the first witness, the Johns Hopkins President drew heavily on his pedagogical skills. As he saw it, the lesson of World War II and "our lack of preparation for it in 1940" was that "regretfully we . . . must continue Federal support of science," but the means employed "might destroy" the "ends in view" if the agency envisioned in the bills under consideration were politically controlled. "Do not put the proposed . . . Foundation in the category of just another Government bureau," he lectured. "Don't assume bureaucrats can do a better job than so-called 'fuzzy scientists.' "

It was Bowman's strictures on this score that Maverick challenged. Striding to the witness stand, the onetime New Dealer proceeded to deliver an old-fashioned stump speech. He characterized Dr. Bowman as "a gentleman and a scholar. . . and a Patriotic American," but snapped that the dynamic geographer "need not be so smug." Maverick was getting "a little tired of . . . some of these bulldozing scientists, piously abrogating[sic] to themselves all the patriotism." He objected to their casting "suspicion on their fellow-Americans who have been shown public confidence by having been elected" to public office. Who, he asked, "was smart enough . . . to appoint Dr. Bowman to numerous scientific missions," and answered, a "politician . . . named Franklin Delano Roosevelt."

Dr. Bowman had said he was against Federal support of science if it meant "political management." Maverick said he would be against such support if it meant "monopoly management." He predicted that if the nine-man board contemplated in the Magnuson bill were in charge of the Foundation, the big corporations would see to it that their scientists dominated it. He warned that the situation thus produced would be fraught with evil because "working for a monopoly for $50,000 a year does not necessarily make a scientist pure, even if he's a pure scientist." The Foundation director appointed by such individuals, Maverick said, would find himself "working for them and not for . . . the people of the United States."

The hearings ended on November 2 but the current session of the 79th Congress would be over and the second one started before Kilgore and Magnuson got around to introducing a compromise measure—a delay brought about partly because the legislators' scientific interests were focused temporarily on Atomic Energy Commission bills, partly because it took time for Senatorial staffs to sift the numerous facts and opinions accumulated during the science legislation hearings.

The immediate outcome of the hearings was an expanded effort by the scientists to expedite legislation. *The New World* (volume one of the history of AEC)[21] offers a fascinating picture of the extent to which the men who built the atom bomb turned lobbyists in 1945-46 in their determination to place the awesome power they had released under responsible civilian auspices. For an even longer period many of those interested in the Foundation exhibited a similar willingness to give time and money to the cause, a number of them treating Washington to the spectacle of astronomers and biologists roaming the halls of the office buildings on Capitol Hill, hobnobbing with Congressmen and talking to reporters. Some exhibited a marked penchant for their new role, their high spirits and personable ways dealing a belated blow to the popular notion of the scientist as forever confined to his laboratory like a mole to its tunnel.

One of their plunges into political activism took place in Baltimore on November 15, 1945, with the formation at Johns Hopkins of the Committee Supporting the Bush Report under the chairmanship of Isaiah Bowman. The Committee's first public action was an open letter to the President extolling the merits of the Magnuson bill. Truman's response was a letter to Bowman, affirming the President's preference for the Kilgore bill. During the closing months of the year Bowman and other members of the Committee were often in Washington, conferring with Bush and his aides at OSRD, with members of both the Kilgore and Magnuson staffs.

To say that the scientist-lobbyists were diligent is not to say that they were unfailingly successful. They stumbled in 1946. The progress of National Science Foundation legislation that year was stymied by confusion, much of it attributable to the scientists themselves.

Introduced on February 21, the long awaited Kilgore-Magnuson compromise—S. 1850—cleared the Committee on Military Affairs and reached the floor of the Senate in early spring. The bill bowed in the direction of the White House by committing the powers of the Foundation to a presidentially named Director. It invited scientific approval by stipulating that "before appointing an administrator," the President must "consult with and receive the recommendations" of a presidentially appointed Board. Praise for this arrangement appeared in the March 29th issue of *Science*, journal of the American Association for the Advancement of Science, a nationwide organization with which some 190 other scientific societies are affiliated. Accompanying it, however, was a stern warning.

Writing as Executive Secretary of AAAS, geologist Howard A. Meyerhoff pointed out that it had taken 7 months to compose the compromise. With elections coming up "for a third of our Senators and all of our Representatives," any further substantial delay could be fatal. "Congress is not going to remain in session any longer than it must," he predicted, adding that without a greater "show of support" from the scientists, S. 1850 stood little chance of passage in 1946.

Time would lend substance to Meyerhoff's concern. One of the sponsors of the compromise was Republican Senator Leverett Saltonstall of Massachusetts. As Saltonstall and Bush were friends, many interested observers assumed that the OSRD Director was reconciled to the Kilgore-Magnuson bill. This view took on strength with the appearance in the May 3d *Science* of an "appeal to the Congress to enact S. 1850," signed by Chairman Bowman and 35 other members of the Committee Supporting the Bush Report. As this plea was made "on behalf of . . . some 5,000 scientists" it seemed to be the "show of support" that the AAAS magazine had called for. But it turned out to be illusory.

On May 15 Democratic Congressman Wilbur D. Mills of Arkansas introduced another NSF bill. Like the Magnuson offering of the year before, H. R. 6448 called for putting the Foundation in the hands of a Board with the Director occupying a subsidiary position. On the 28th a subcommittee of the Interstate and Foreign Commerce Committee of the House began 2 days of hearings. Testifying at the opening session Vannevar Bush called H.R. 6448 "an excellently drawn piece of legislation," one that would "fulfill the needs of the country better than any other piece of legislation I have seen for the purpose." Asked what he thought of attempts in the Senate to compromise the differences between the original Kilgore and Magnuson measures, Bush replied that "one . . . ought to be willing to give and take on any issue, unless he feels that the public interest would be injured if he went too far." He felt that in this instance "one way of doing things might work better than another. However . . . I am not one who thinks the entire program would be wrecked by the way in which a number of these issues are determined."

Following Bush on the witness stand was Dr. Homer W. Smith of New York University's School of Medicine. Dr. Smith had served as secretary of the medical committee that had helped Bush prepare *Science, the Endless Frontier.* As a member of the Committee Supporting the Bush Report, the New Yorker had signed the "appeal to Congress" to enact the Senate compromise. A note of "yes-and-then-again-no" running through his testimony indicated that he was taken aback by his colleague's words, perhaps because of Bush's unreserved endorsement of the Mills bill, perhaps because of the OSRD Director's failure to say at just what point the give-and-take on the issues should be cut off. Dr. Smith doubted that the President would accept H.R. 6448. He wished, however, that he would. Like Bush, he would prefer an all-powerful Board to an all-powerful Director.[22]

In the pages of *Science*[23] Meyerhoff sounded the alarm. He wrote that in calling H.R. 6448 "an excellently drawn piece of legislation," Bush spoke with authority since he himself had drafted it. Meyerhoff said that an associate of Bush had not only got Congressman Mills to sponsor the bill but had seen to it that only scientists favorable to H.R. 6448 were summoned to testify at the hurriedly called hearings on it. The AAAS Secretary did not name the "associate," but no doubt many readers recognized the reference to John H. Teeter, the enterprising promotion man who during the preceding year had been instrumental in bringing Bush and Senator Magnuson together. Meyerhoff charged that by interjecting the Mills bill into the picture Bush had "seriously jeopardized" the passage of the Kilgore-Magnuson compromise or any other National Science Foundation bill by the present Congress.

That Bush resented being cast in the role of villain of the piece is shown by some of his correspondence during this period.[24] The protests of his friend and ally, Isaiah Bowman, were even more vehement. "Dear Van," he wrote in June, "Meyerhoff has, in recent months, tried to steal the show in his reports in *Science* and make it appear that the AAAS is doing the real work of supporting the right plan for science legislation." In the outspoken geographer's opinion these tactics constituted "a deplorable misuse of the journal of a national association of scientists." Bowman thought that "*Science* should report objectively upon movements of this kind and not take a position and hang on to it, battling the efforts of others." The Johns Hopkins President cited with special distaste an article in the magazine by another writer who, in Bowman's view, had completely ignored strong signs that the Kilgore-Magnuson compromise would never win the approval of both Houses of Congress.[25]

Apparently this possibility was on Dr. Bush's mind as well. At an informal meeting in Boston toward the end of the year, Karl Compton confided to 20 other scientists his understanding that the introduction of the Mills bill had been "due to the belief of Dr. Bush that S. 1850 would not pass the House." Compton added that henceforth Bush could be counted on to support measures "along the lines" of the Kilgore-Magnuson compromise.[26]

Whatever Bush's motives in encouraging the introduction of H.R. 6448, his doing so helped produce the results that Meyerhoff feared. In July Herbert Schimmel of Senator Kilgore's staff told a Budget Bureau officer that because there was "no meeting of minds among the scientists," the House Foreign and Interstate Commerce Committee was about to "bury" all NSF legislation "for this session."[27] Dr. Meyerhoff was the recipient of a similar comment from Republican Congressman John W. Heselton of Massachusetts. Heselton said that "the feeling in the House was that until the scientists themselves got together on the kind of organization they wanted, the members of Congress should do nothing."[28] The Mills bill never emerged from its legislative committee. As for the Kilgore-Magnuson compromise, it passed the Senate on July 3, only to die in the House when on August 2 the 79th Congress adjourned *sine die* and most of the members hurried home to bid for reelection.

In the light of subsequent events it is difficult to specify just how much the faltering and disunity among the scientists contributed to the failure of NSF legislation in 1946. If it is correct to say that some of them bungled rather conspicuously that year, it is also correct to say that the members of Congress themselves were the principal authors of what were soon to become the follies of 1947.

Shortly before the end of 1946 the country's largest organization of its kind, the 26,000-member American Association for the Advancement of Science, held its annual meeting at the Statler Hotel in Boston. The dismal status of NSF legislation was a source of much talk and soul-searching. There was a feeling among the delegates that the scientist-lobbyists had made mistakes, that new and better tactics were needed. No one was surprised when on the 29th the Council of the organization unfolded a plan for hastening action by inviting all AAAS affiliates and certain other groups to elect representatives to an Inter-Society Committee for a National Science Foundation.

Two months later, in Washington, the new group began life with 140 members from 75 societies. To steer the work of the Committee, the organizers set up an executive subcommittee headed by President Edmund E. Day of Cornell University as chairman, astronomer Harlow Shapley of Harvard as vice chairman, and Dael Wolfle of the American Psychological Association as secretary-treasurer. In a transparent effort to keep peace in the scientific family they named Isaiah Bowman to the executive panel. When the Johns Hopkins president begged off "because of the pressure of other duties" they expressed the hope that he would suggest as a substitute some other member of his Committee for Supporting the Bush Report. The available records show no replacement for Bowman. They do show that many members of his Committee were also active in the AAAS Committee.

Early in 1946 Democratic Senator Elbert D. Thomas of Utah, AAAS member and onetime professor of political science, had told 19 scientific organizations meeting together in Philadelphia that if the current Congress passed no NSF legislation he himself would offer a bill at the next session. Thomas' bill—S. 525—appeared on February 7, 1947. The same day saw the introduction of S. 526, subsequently known as the Smith bill after its chief sponsor, Republican Senator H. Alexander Smith of New Jersey. In the House a third NSF bill had already been presented and others would emerge as what Harry Truman later called the "do-nothing 80th Congress" set out to do something for American science.

Where the administrative form of the proposed agency was concerned, Senator Thomas' measure and one of the House bills duplicated the Kilgore-Magnuson compromise of the year before. The others either provided for a Board-controlled Foundation or for one managed, like the Atomic Energy Commission, by a corps of full-time commissioners. By March all of them were under the scrutiny of legislative committees, and on the 5th of that month the heads of the Inter-Society Committee assembled to map strategy. One point of agreement among them was that to date most public testimony dealing with the administrative issue had come from a few eminent scientists and that the voice of these individuals was not necessarily the voice of American science as a whole. Out of this thinking came the group's first policy decision. The Inter-Society Committee would of itself take no position on the matter. Instead it would sound out a cross-section of the country's scientific workers and convey to Congress whatever appeared to be the majority view.

Back in 1945 the parent organization, AAAS, had solicited the opinions of a broad segment of its membership on the differences between the original Kilgore and Magnuson proposals. On most of the issues the answers were clear enough. Those concerned with how NSF should be run, however, were open to conflicting interpretations. At fault was the equivocal manner in which the questions covering the problem had been phrased—a situation that had prompted Senator Kilgore's aide, Dr. Schimmel, to quip that in polling opinion even scientists should try to be scientific.

The questions framed on this point by the Inter-Society leaders were carefully designed to attract explicit answers. By March 19 replies were on hand from most of the 140 Committee members. Only 18 percent of them favored a Board-controlled NSF. Sixty-three percent opted for a single administrator and 18 percent for an AEC type of organization.

Encouraging to the opinion samplers was the discovery that these percentages agreed with those elicited in two additional surveys, one conducted among the scientists in California, the other across the Nation by the Federation of American Scientists. Convinced that the three tallies, taken together, were representative, the Inter-Society leaders informed Congress that a "two-to-one majority" of American scientists preferred the single-administrator structure contemplated by Elbert Thomas' S. 525.

As Dael Wolfle would point out in his first annual report as secretary of the Inter-Society Committee, the legislators paid "little attention" to these findings.[29] During the summer both Houses passed an amended version of H. Alexander Smith's S. 526. This measure lodged all of the authority of the Foundation in a 24-member board. As Truman had repeatedly expressed his dislike for such an arrangement, the question arises as to why the legislators sent the Smith bill to the White House.

Different observers hazard different guesses. Some believe that the Congressmen assumed that the President's known desire to see NSF under way would impel him to sign in spite of his objections.

Howard Meyerhoff ventures the not unconvincing thesis that by late July 1947 Congress was just plain tired of National Science Foundation legislation. "The Senators and Representatives," he speculates, "were weary of scientists coming up the Hill and bothering them. I imagine they hoped Truman would bring himself to approve. That way they could get us out of their hair."[30]

But the scientists weren't in the President's hair. As Congress was in recess when the bill got to him, he was under no obligation to explain what was technically a pocket veto. Nevertheless, he issued a memorandum of disapproval. He stated that the organization provided by the Smith bill violated basic principles of Government responsibility, indicating, on the part of its framers, a lack of faith in the democratic process.

Thus fell the first National Science Foundation legislation to reach the White House, and the Foundation itself now looked farther away than ever. But in his memorandum of disapproval Truman used strong language to reaffirm his eagerness to see it established. Like a breath of air directed at a dying fire, his words kept flickering hopes alive. Dispatched to Congress on August 6, the President's message was unmistakable: He was saying to both the legislators and the scientists, "Try again!"

They tried. During the remaining months of 1947, however, the only pertinent development of consequence was the issuance by the White House of *Science and Public Policy*, a five-volume survey of the country's scientific problems compiled under the direction of presidential assistant John R. Steelman. In those portions dealing with the need for a National Science Foundation, the Steelman Report emphasized the importance of putting the proposed agency under a single administrator responsible to the President.

Although all NSF bills considered on the Hill in 1948 would have been acceptable at the White House, none of them survived the adjournment of the 80th Congress at the end of that year. It was left to the next Congress to bring the long controversy to a close—and even its efforts might have failed at the last moment had it not been for a legislative fluke.

The last maneuvers began early in 1949. The Senate passed and sent to the House S. 247, sponsored by Thomas of Utah and others. The House took under consideration a companion measure, H.R. 4846, introduced by Democratic Congressman James Percy Priest of Tennessee.

A new feature of the Priest bill was that it imparted to the Foundation the dual authority under which it would eventually function. Using language unknown to previous measures, H.R. 4846 defined the Foundation as consisting "of a National Science Board . . . and a Director," and gave each of them roughly the same powers.

It is the recollection of Dael Wolfle that this scheme for resolving the old management difficulty had been under consideration for years. Wolfle no longer remembers precisely who developed the idea—possibly Harold Smith while Director of the Bureau of the Budget, "perhaps also . . . others." Wolfle does remember a long conference on the matter, sometime in the winter of 1948-49, in the office of Elmer B. Staats, then with the Budget Bureau, now Comptroller General of the United States. Taking part in addition to Wolfle and Staats were the other top officers of the Inter-Society Committee, Chairman Day and Vice Chairman Shapley, and one or two other members of the Bureau staff.

"After we had agreed on the dual structure and had written out appropriate wording for inclusion in a bill," Wolfle recalls, "someone suggested that we try out the idea on Dr. Bush. We phoned and found that we could see him almost immediately. Accordingly, Dr. Day and I called on him, and Bush not only pronounced the plan workable but went on . . . to add something like 'and we will get a distinguished and well known scientist, such as Dr. Conant, to serve as chairman of the board and someone like Alan Waterman to serve as Director' "—a suggestion that time would prove uncannily prophetic. Having obtained Bush's approval of the plan, Wolfle took it to Congressman Priest.[31] Staats had already shown the idea to other members of Congress, and the dual management plan was soon incorporated into H.R. 4846.

As none of the differences between the Priest bill and the NSF measure in the Senate appeared to be irreconcilable, hopes for passage ran high. By April 1949 prospects were so bright that the Budget Bureau, which acts as a midwife to new Federal agencies, began making plans for putting the National Science Foundation into operation.

But hurdles remained and one of them turned out to be almost insurmountable.

In the Senate a bill has only to obtain the blessing of a legislative committee to become eligible for debate on the floor. In the other branch of Congress the obstacle course is longer. From a legislative committee an affirmatively reported measure goes to the Rules Committee. In every Congress the House provides parliamentary devices under which a bill can be brought out of the Rules Committee by floor action. These procedures, however, are rarely resorted to. In most instances a House bill reaches the floor only after a majority of the powerful Rules Committee votes to discharge it, a process spoken of as giving the measure "a rule for consideration."

On June 14 the Priest bill went to the Rules Committee—and 2 months later the *Washington Post* was lamenting editorially that it was still "stuck" there. Under the headline, "Pigeonholed Science," the newspaper implored Congressman Priest "to exert every influence" to pry it out.

In truth the struggle to do so was barely started. In August, at the urging of Dael Wolfel, Priest talked to every member of the Rules Committee. Reports reaching the Budget Bureau were that the opposition "within Rules" was spearheaded by James W. Wadsworth of New York and other Republicans "on economy ground." Wadsworth was also described as having been "influenced" by former Academy President Frank Jewett, whose dislike of the whole idea of an NSF remained unshaken.

An Ohio scientist, having got in touch with a Rules Committee member from his State, informed Wolfle that the Priest bill was "a dead duck." From Ithaca, N. Y., Chairman Day of the Inter-Society Committee made a special trip to Washington in an effort to persuade Wadsworth and other friends on the Rules Committee to change their minds, a futile effort as it turned out. "Today," a Budget Bureau officer reported on August 18, "Mr. Wadsworth . . . issued a statement . . . that the bill is dead for this session because of the opposition generated by economy influences."

Neither the scientists nor their cohorts in Congress gave up. Since the 1947 veto Vannevar Bush had kept more or less aloof from the fray, but in October 1949 he was conferring frequently with Wadsworth. On the 13th Bush wrote Harold Smith's successor as Budget Director, Frank Pace, Jr., to propose a scheme that he believed would bring the New York Congressman around. Bush's suggestion was that Wadsworth's desire to economize be met by a clause in the NSF bill stating that its enactment would not result in any increase in the total amount spent by the Federal Government on scientific research.

There were no takers for this proposal at the Budget Bureau. Pace said as much in a letter to Wadsworth. He described himself as "in agreement" with the New York Congressman "that we must examine critically every request for new expenditure programs to make certain of their essentiality." He added, however, that applied research and development accounted for the bulk of the Government's expenditures for science and that the definitely needed basic science programs NSF would be called on to support would be relatively inexpensive.[32]

Pace's letter was sent in late November. By this time the first session of the current Congress had adjourned with the Priest bill still tied up in the Rules Committee. There, undoubtedly, it would have stayed, to die at the end of the second session, were it not for the existence in the House at this point of a parliamentary device called the 21-day rule.

In most Congresses the only floor action capable of bringing a bill to debate without the approval of the Rules Committee requires getting the signatures of a majority of the entire House on a petition—a cumbersome and time-consuming process. Fortunately for the proponents of the NSF measure, the 81st Congress had adopted a simpler method. Under the 21-day rule the chairman of the legislative committee that had reported the bill favorably could bring it out of the Rules Committee 21 days after demanding its release from that body.

In this instance the legislative chairman was Democratic Congressman Robert Crosser of Ohio. On February 27, 1950—9 months and 13 days after the Priest bill reached the Rules Committee—Crosser raised it from its legislative grave by invoking the 21-day rule. As this parliamentary device was rescinded by the next Congress, it is easy to appreciate the comment of Lyman Chalkley in his unpublished *Prologue to the U.S. National Science Foundation* that "the Foundation uniquely benefited by this short-lived procedure."*

Once the NSF bill reached the floor of the House, considerable debate followed. Several amendments were voted, some of them destined to produce difficulties in later years. And in March differences between the House and Senate measures were ironed out in conference.

Final passage came in the House on April 27, in the Senate the next day. At which point the bill—officially designated as S. 247 although much of its terminology derived from the amended House bill H.R. 4846—went to the President. Truman was out of town at the time, on a speaking tour. Apparently he had no objections to the idea of a Foundation jointly run by Board and Director. On May 10, as his special train sped across Idaho, he signed S. 247 into law.[33]

---

* In 1965 the 89th Congress revived the 21-day rule, and the 90th Congress revoked it. It is worth adding, as a matter of interest, that back in 1948 another NSF bill, after passage by the Senate, perished in the House Rules Committee. If "unanimous consent for immediate consideration" can be obtained, certain types of bills can be passed by the House without Rules Committee approval. In these circumstances, however, no floor debate is permitted.

Chapter 3. Old Fears and New Ideas

[1] *Science in the Federal Government.* Cambridge, Mass., 1957, pp. 104 and 376.

[2] Ltr., Daniel J. Kevles to author, Feb. 20, 1974.

[3] Kevles, ibid.; Carroll Pursell, Jr. "The Science Advisory Board, 1933-35," *Proc. Am. Philos. Soc.*, Dec. 1965.

[4] See, for example, Kevles, "George Ellery Hale, the First World War, and the Advancement of Science in America," *Isis*, Winter 1968, pp. 427-437.

[5] Conant, *My Several Lives.* New York, 1970, p. xv.

[6] John Phinney Baxter III, *Scientists Against Time.* Boston, 1946, pp. 13-14.

[7] Price, *The Scientific Estate.* Cambridge, 1965, p. 65.

[8] Personal interview.

[9] Conant, ibid., pp. 235-236.

[10] Stewart, *Organizing Scientific Research for War.* Boston, 1948.

[11] June 7, 1946. Isaiah Bowman Papers, General Correspondence, Frieda C. Thies Ms. Room, Milton Eisenhower Library, Johns Hopkins University.

[12] "National Science Foundation Legislative History. Prints and Bills," 3 vols., Law Library, Office of General Counsel, NSF. On the early Kilgore bills, see *Science*, vol. 97, pp. 375-377, 442-443, 461, 482-483, 486-488, 510-511, 528, 552-555; vol. 98, pp. 151-152.

[13] Interview.

[14] "Letters," *Science*, vol. 183, pp. 798, 800.

[15] First issued in July 1945; reprinted by NSF, 1960.

[16] Memo, Price to Miles: Bush-Kilgore Developments, July 20, 1945, BOB 39.32 (E8-21/44.2), NA.

[17] Ag. 7, 1945, AEC Records, Bush-Conant Files, NA.

[18] Personal interview.

[19] For the testimony quoted, see *Hearings on Science Legislation (S. 1297 and Related Bills)*, 79th Cong., 1st Sess., 1945. Contents of bills mentioned in this chapter are taken from "National Science Foundation Legislative History. Prints and Bills;" or from related congressional committee reports.

[20] Newspaper Clipping, *Worcester* (Mass.) *Gazette*, Jan. 3, 1949, in Bowman Papers at Johns Hopkins.

[21] Richard G. Hewlett and Oscar E. Anderson, Jr., *The New World, 1939/1946*, University Park, Pa., 1962, pp. 445-448.

[22] *Hearings . . . on H.R. 6448*, 79th Cong., 2d Sess., 1946.

[23] June 7, 1946, pp. 687-688.

[24] Bush to Bowman, June 18, 1946, Bush Papers, LC.

[25] Bowman to Bush, June 7, 1946, ibid.

[26] Summary Report of Informal Meeting Called by Dr. Harlow Shapley. BOB 39.32 (E8-33/44.2) NA.

[27] Memo, Merritt to Patterson French, July 19, 1946, ibid. (E8-21/44.2).

[28] Phone interview with Meyerhoff.

[29] *Science*, Dec. 5, 1947, pp. 529-533. The activities of the Inter-Society Committee rest largely on this document, supplemented by a small collection of records kindly supplied by AAAS.

[30] Interview.

[31] Ltr., Wolfle to author, July 25, 1973.

[32] Memo, Carey to Staats: Dr. Bush's Proposal on NSF Legislation, Oct. 20, 1949; memo, C. Spencer Platt to Ralph J. Burton: Dr. Bush's Proposal on NSF Legislation, Oct. 24, 1949; and ltr., Frank Pace, Jr. to Hon. James W. Wadsworth, Nov. 21, 1949. Coverage of efforts to force the bill out of the rules committee rests on these and other memos and letters in BOB 39.32, NA; and on Lyman Chalkley's unpublished *Prologue to the U.S. National Science Foundation.*

[33] *New York Times,* Nov. [illegible], 1950. For progress of final bills in Congress, see *Cong. Rec.,* 81st Cong., 1st Sess., pp. 2411, 2422, 2426-27, 2429, 2431, 2435, 2436, 2442, 2444-46, 2449, 2451, 2516-19, 4067, 4069, 5899-5905, 5907-08, 5960, A1810, A1863, A3158, A4275; and 81st Cong., 2d Sess., pp. 2522-23, 2578-81, 2597, 2602, 4067, 4069, 5899-5906, 5960.

Chapter 4

Choosing the Board and a Director

Under the NSF Law of 1950 the President had to install the National Science Board first and then weigh its members' suggestions for a Director before filling that post. The enabling act also provided guidelines for choosing the Board members. They were to be "eminent in . . . the basic sciences, medical science, engineering, agriculture, education, or public affairs, and . . . so selected as to provide representation of the views of scientific leaders in all areas of the Nation."

In addition, the law requested the President to "give due consideration" to recommendations from the country's scientific and educational societies. A number of such recommendations found their way to the White House. And to compare the list proffered by a large scientific organization with the names Truman actually submitted to the Senate in early November is an enlightening exercise. The comparison shows that the Man from Missouri was determined to give the first National Science Board a more democratic cast than the scientific community wanted it to have.

As confirmed by the Senate in December, the appointees came from all parts of the country and represented all of the major scientific disciplines. There were two women and two blacks. Twenty of the members held academic posts and in terms of status their employers ran the gamut. Among them were prestigious private schools like Harvard and Columbia. There were also State universities and land grant colleges, a little-known college in the southern hinterland, black institutions, and Roman Catholic institutions. Even political considerations were not altogether disdained. The National Democratic Committeeman from a certain State breakfasted with the President, and a few days later one of his constituents received an appointment to the Board. Thanks to these Truman touches the first National Science Board was more evenly divided between "haves" and "have-nots" than any of its successors. Once the Board began functioning, however, time would tend to mute these diversities as little by little interest coalesced instead around those members who were engaged in administration, on the one hand, and the working scientists, on the other.

Press reaction to publication of the 24 names in the late fall was on balance a favorable one, although some scientific journals complained that there were proportionately too many chemists and that the engineering arts had been skimped. Oddly enough none of the editors seems to have mentioned the absence from the list of the man generally regarded as the father of the National Science Foundation.

In his autobiographical *Pieces of the Action,*[1] Vannevar Bush offers an interesting explanation of how it was that he came to be the man who wasn't there. A few weeks before public announcement of the appointees, he sat next to Truman at an Armed Services Day dinner at the Mayflower Hotel in Washington. When the subject of NSF came up, Bush seized the opportunity to ask the President not to name him to the National Science Board.

"I know my name is on the list," he said, "but I wish you would leave me off." Pressed to justify his request, he pointed out that since the opening of World War II he had been "running about everything scientific" around Washington and he assumed that people were "getting tired" of seeing him do so. "If you put me on the board," he told the President, "they will elect me chairman, and I do not think that the body of scientists are going to like this continuation of one man in the top post. . . ."

According to Bush, Truman gave him a friendly poke in the ribs and agreed to "leave me off."

Under Roosevelt, during the war years, Bush had been for all practical purposes the science adviser to the President. In *Pieces of the Action* he ventilates with engaging candor his annoyance at Truman's failure to use him in the same capacity. Personally he and the President got along well enough. When Truman said, "Van, you should be a politician," the former OSRD Director snorted, "Mr. President, what the hell do you think I was doing around this town for 5 or 6 years?" Bush thought highly of Truman, called some of his Presidential decisions the acts of a statesman, and blamed his own unsatisfactory relations with the Truman White House not on its main occupant but on the "palace guard," meaning John R. Steelman, Truman's vigorous and science-minded special assistant and the linchpin of his staff.

In the course of a conversation with Truman in the Executive Office Bush made a pointed reference to Steelman's authorship in 1947 of the 5-volume survey, *Science and Public Policy.* "[W]hen F.D.R. wanted a report on postwar science, he called on me," he said. "When you wanted a report, you called on Steelman." Speaking with characteristic frankness, Bush added that he could only interpret the President's practice of turning to Steelman for scientific guidance as a lack of confidence in himself. Truman's response, as Bush subsequently described it, was "a long harangue to the effect that he had full confidence in me . . . was going to lean on me just as much as he ever had, and so forth and so on." But in point of fact Truman was not leaning on Vannevar Bush.

In the late 1940's James E. Webb was in a good position to observe the genesis of the Bush-Steelman standoff. Webb is best known as Administrator of the National Aeronautics and Space Administration in the 1960's when NASA's engineers, scientists, and astronauts made commuting to the Moon commonplace. His first close look at Government science problems, however, came years earlier during his Directorship of the Bureau of the Budget from 1946 to 1949.

"You have to remember," Webb says, ". . . that when a person works in the White House, whether it's Steelman or anyone else, when they are given a job to do or when they reach for work in a certain field because they're interested in it and ask the President to let them do the work, they tend to look for colleagues and people who will work for them. And I don't believe Dr. Bush had any strong feeling of approval of the fact that Steelman was taking a fairly strong and leading role toward the development of White House policy for science."

In those days, according to Webb, one of the functions of the Bureau of the Budget was to invite the attention of the President and his staff to "emerging problems." With this end in view the Bureau compiled as an appendix to one of its budgets "a special section on the research and development activities of the Government which for the first time made them known and generally understood. And Dr. Steelman, among others, took a very active interest in the fact that here was an area that was clearly emerging as an important area for policy, and so he prepared his long report to the President with respect to things that should be done. Now I think the scientists sort of felt that they themselves wanted to be intimately involved in the preparation of this kind of report, or wanted to do it all where it involved a powerful office like the President. So there was tension."[2]

Only superficially was it a competition for power at the Presidential level between two capable and dedicated men, Bush and Steelman. Behind their rivalry lay a subtle but significant difference of outlook as to how the Federal Government should manage its scientific shop. James L. Penick, Jr. and his collaborators, in their documentary survey of science-Government relations since 1939, attribute the publication of the Steelman Report to dissatisfaction on the part of "a small group of men in the office of the Chief Executive" with aspects of Bush's *Science, the Endless Frontier.* The group mentioned by Penick et al. included some of the members of the President's Scientific Research Board, set up within the White House to assist Steelman in the preparation of *Science and Public Policy.* These men several of them top-level Government officials, discerned in the Bush

Report "an underlying anti-democratic sentiment which they believed should be countered." By their lights "Bush had dealt unfairly" with the research carried on by Government employees and "had magnified the importance of university science."[3] Hence the statement in the Steelman Report that "recommendations for a national policy on science must necessarily reflect many considerations but remotely connected with the laboratory."[4]

The Bush Report tended to view science as an end in itself. The Steelman Report applied to it a political prism, placing more emphasis on the role of science as a means to an end, the end being the public welfare. The Truman Administration, by publishing its own survey of postwar science practically on the heels of the Bush Report, brought into focus a philosophic dichotomy on the subject that, coming to view at intervals in the future, would run through the story of the National Science Foundation like a leitmotiv in a Wagnerian opera.

Steelman, a sociologist and economist, had held a variety of responsible government positions when in 1946 he accepted a bid to act as Truman's top special assistant. In the fall of 1950 he did much of the work involved in providing the President with the information he needed to make his selections for the National Science Board. Steelman's explanation of why the father of the Foundation was not included dovetails nicely with Bush's own version.

"Quite frankly," he says, "I thought it unwise to put his name on the list for the reason that a number of scientists whom we had approached about sitting on the Board said they wouldn't serve if Bush were a member. Their reasons were that he was 'overbearing,' that if he were on the Board he'd take charge of everything." Steelman quotes one scientist as remarking plaintively, "Van Bush is one of those fellows who sweeps all before him."[5]

Truman himself called the organizing meeting of the National Science Board at the White House on December 12, 1950. It was a cold and cloudy day in Washington. The remnants of a recent snow were graying in the rose garden beyond the arched windows of the Cabinet Room as the members set about their work. The first order of business culminated in the election of Conant of Harvard as chairman of the Board and Edwin B. Fred, president of the University of Wisconsin, as vice chairman.

Steelman had dropped in briefly at the start to extend the official greetings of the White House, and the President arrived at noon to give his talk. He was well acquainted with some of the members. "I might have known Det Bronk would be here," he joshed at the new president of Johns Hopkins. "Dr. Bronk is always on hand when the Government is giving away money." He asked Chairman Conant what the members had been "up to." On learning that they were preparing to frame criteria for the selection of a Director, Truman said, "Oh don't bother with that. There's only one criterion. He must get along with me, and I already have my man picked."[6]

In his speech he reviewed the long battle over NSF legislation. Dr. Sophie B. Aberle of the University of New Mexico noted that the President's face was drawn, his manner abstracted. Plainly his mind was not in the Cabinet Room. It was thousands of miles away, with his country's imperiled troops in Korea.

He left at 12:20, and the members went to lunch. Where, none could later remember, except that there was a piano on the premises and Professor Marston Morse entertained his colleagues by playing a few tunes. Back at the White House, they elected nine of their number to an Executive Committee whose members would subsequently choose Dr. Bronk as their chairman.

During the morning session Conant had named eight members to a Special Committee on Recommendations for a Director. He told them not to think of themselves as a nominating committee. They were simply to offer suggestions for the Board as a whole to mull over.

For its second monthly meeting on January 3, 1951, the Board convened in Room 331 of what is now called the Old Executive Office Building in Washington. Chairman of the Special Committee on Recommendations for a Director was Chester I. Barnard, president of the Rockefeller Foundation. Barnard reported that his group had first drawn up a list of 40 names and had then pared it to 11. The Board members chose to discuss the qualifications of all 40. The upshot of this long and no doubt lively exchange was that three names were dropped from the Special Committee's short list and two names from the Committee's long list went onto the short one. Taking the reconstituted short list to be the Board's official slate (a tentative one, as it turned out) the members resorted to written ballots to indicate their preferences. This action yielded ten names with Detlev Bronk in first place, followed by Harvard biochemist A. Baird Hastings and physicist Lloyd Berkner of the Atomic Energy Commission in second and third places. Seventh place went to Alan T. Waterman of the Office of Naval Research, an item of interest because of impending developments.

At this point Edward L. Moreland, executive vice president of Massachusetts Institute of Technology, noted that all but two members had named Bronk as their first choice, and those two had ranked him second and fourth respectively. An upcoming motion passed unanimously. It required "the record to show that Mr. Bronk was regarded by the Board as the outstanding candidate for appointment as Director." Another motion instructed Conant to submit the three top names on the list to the President, making it clear that Bronk was the Board's overwhelming preference.

Conant performed this errand in person but at the next Board meeting on February 13-14 Bronk asked that his name be withdrawn. As this change left only two Board recommendations at the White House, the members voted to submit seven additional names. Their final list, as presented to Truman, consisted of Hastings, Berkner, chemist Roger Adams of the University of Illinois, bacteriologist Ira L. Baldwin of the University of Wisconsin, biologist Lowell T. Coggeshall of the University of Chicago, chemist Reuben G. Gustavson of the University of Nebraska, physiologist C. N. H. Long of Yale Medical School, Waterman of ONR, and metallurgist Clyde E. Williams of Battelle Memorial Institute.[7]

Chester Barnard and his Special Committee on Recommendations had composed a "'pattern' of qualifications" that they hoped the President would follow in selecting a Director. The Committee thought it "highly desirable" that, among other things, the candidate be either an able practicing scientist or an experienced science administrator; that he possess an insider's understanding of the country's institutions of higher learning and research; that he know how to negotiate contracts, control budgets, conduct conferences, and supervise the work of "high-class personnel;" and that he be "adept in the management of 'public relations'."

It is not clear that these criteria were ever presented formally to Truman, although Conant probably conveyed the gist of them during his personal call at the White House on January 5. In any event Truman and Steelman had every intention of finding a Director the Board members could respect. Their feeling, however, was that of all the qualifications listed by the Barnard Committee the most important was "that the candidate have a good understanding of the practices . . . ways and attitudes of government officials . . . departments and bureaus."[8]

Dictating this emphasis were the circumstances under which NSF had begun life after its long gestation on the Hill. At the time of the passage of its enabling act, the Foundation was already a Johnny-come-lately where many of the functions everyone had assigned to it 5 years earlier were concerned. In 1945 Vannevar Bush had recommended that when OSRD closed shop, its surviving contracts for medical research be shifted to the Foundation. From the beginning the members of the medical committee associated with the Bush Report were less than enthusiastic about this idea. As the years passed and no NSF appeared they easily persuaded Congress to convey the contracts to the National Institutes of Health instead. This action jumped NIH's expenditures for scientific research in academic and industrial laboratories from $850,000 in 1946 to $4 million in 1947 when the transfer of contracts from OSRD was completed.[9] It also deprived the Foundation of what, had it come into existence at an earlier date, might have been an extensive medical program.

During the long hassle over NSF legislation two other events—the establishment of the Atomic Energy Commission and the Office of Naval Research—had further limited the area in which the Foundation could operate. AEC took all nuclear research and development as its province. A clause of the NSF law recognized this situation by permitting the Foundation to finance work in nuclear science only after obtaining the concurrence of the Atomic Energy Commissioners. ONR, since its inception in 1946, had taken up the slack in many other fields of research.

Financially speaking, the newborn NSF was barely breathing. In 1945 Vannevar Bush had said that the Foundation would need at least $130 million a year.[10] In 1950 the figures being bandied about Washington were considerably more modest. Although the House antispending elements had failed in their effort to inter the NSF bill in the Rules Committee, they managed to put their imprint on the law itself. As finally approved, the enabling act put low ceilings on the Foundation budget. Congress, in making its appropriations, was instructed "not to exceed $500,000 for the fiscal year ending June 30, 1951, and not to exceed $15,000,000 for each fiscal year thereafter."

By the time the legislators got around to considering money for the new agency in August 1950, the Korean war was well under way, the economy forces in Congress stronger than ever. To permit the Foundation to organize and set up operating procedures, Truman recommended an appropriation of $475,000 for what remained of the fiscal year—and the House Appropriations Committee promptly reduced his request to zero. Its announced reason for this drastic action was that "new programs which will not provide early aid to our defense effort should not be activated at this time."[11] Fortunately for the new agency, a more sympathetic Senate Appropriations Committee reported out a bill, under which, as passed in October, the Foundation received $225,000 to get started on. Thus did events bear out the previously expressed fears of a Budget Bureau officer that Vannevar Bush's "endless frontiers" were becoming "abruptly finite" and his Foundation fading into "a rather puny creature."[12]

So markedly puny was it that Truman and Steelman were convinced that whatever other qualities its first Director might have, he "must above all know his way around Washington." Without such a background the most brilliant of administrators might trip over the silken cords binding one Federal bureau to another and to the White House and Congress.

By early 1951 the President and his assistant had three individuals in mind. Their first choice was Karl. T. Compton, president of Massachusetts Institute of Technology from 1930 to 1948, later Chairman of the Corporation there. Experimenter in photoelectricity, radar, ionization of gases, ultraviolet spectroscopy and electric arcs—capable administrator and man of the world—Compton in the early 1930's had chaired the first body of scientific advisers to sit in the White House. During most of the second World War he had run the Office of Field Services, a major branch of OSRD. His qualifications were high, but Truman and his assistant were doubtful of his willingness to abandon his duties at MIT for a fulltime governmental post. For this reason their offer to him was an informal one. "I don't suppose you'd be interested," said Steelman, and Compton pleaded his desire to remain where he was. The same informal question was put to President Conant of Harvard with the same results. Both Conant and Compton, however, expressed unreserved approval on being informed that the President was giving serious thought to Alan T. Waterman.[13]

During the war, Waterman had served as Karl Compton's deputy at OSRD. Indeed it was Compton who had persuaded Yale University to release Waterman from his duties as a physics professor to assume his wartime duties. Since 1946 he had been Deputy Director and Chief Scientist of the Office of Naval Research. To the surprise and delight of the scientific community, ONR had not only sponsored a great deal of basic research in the universities but had also allowed the beneficiaries of the contracts involved a high degree of freedom in their investigations. Many scientists considered Waterman largely responsible for these happy arrangements, and therefore a "logical choice" for the Directorship of NSF.

Persistent scuttlebutt in Washington had it that the job of nursing the "puny" Foundation through its growing pains looked so unattractive that Steelman offered the post to "some 13 individuals" before finding a taker. "Pure fiction," would be Steelman's comment on this gossip years later. "If 13 guys had turned me down, I'd remember it."[14]

What he does remember is that the first formal offer of the Directorship went to the Chief Scientist of ONR—and Dr. Waterman said "yes."

Truman made up his mind about Alan Waterman early in 1951. Not until March 9, however, was the appointment made—a delay growing out of the Cold War mentality permeating official Washington at that time.

Under way was the era that has taken its name from the late Republican Senator, Joseph R. McCarthy. Just what was wrong with the American china shop in those days that this bull from Wisconsin was able to wreak so much havoc in it over a space of less than 5 years is a question that may never be satisfactorily answered. Suffice it to say, the period was an uncomfortable one for the National Science Foundation to begin life in, as President Conant of Harvard discovered late that winter when he received an urgent summons to the White House in Washington.

By this time Truman's interest in Waterman was common knowledge. Waterman himself had learned of it from a friend who had picked up the information in an Australian newspaper.[15] None of the going rumors, though, had prepared the Chairman of the National Science Board for the revelations awaiting him in the Executive Offices at the White House.

"There," Conant wrote later, "I met the man who was acting as special counsel to President Truman. He said in effect that the security check on Waterman was unsatisfactory; the appointment could not be made."

Conant was "flabbergasted." At the Office of Naval Research "Waterman had had access to all sorts of highly classified material" for years. Conant knew him well; so did his friend, Dr. Bush. What could possibly have gone wrong?

"The trouble," he was informed, "is with his wife." The file on hand at the White House showed that on two occasions Mary Waterman had "been to the Soviet Embassy . . . for tea."

This statement was almost more than anti-Communist Conant could bear. He exploded. To him it seemed "ridiculous" that "such a trivial report" could prevent an "excellent" appointment. "So it may seem to you," he was told, "but with the atmosphere what it is on the Hill, we cannot proceed with this appointment unless you can personally guarantee the man and stand ready to give full public endorsement."

Conant gave "the assurance, and the matter proceeded as scheduled."[16]

Had Mary Waterman known of this incident when it occurred, she too would have been flabbergasted. As Chief Scientist at ONR, her husband from time to time attended receptions at the embassy for visiting Russian scientists. His wife accompanied him. Once, when Waterman himself was unable to be there, she went alone as his surrogate. "But my husband always cleared those visits with the Navy security officer," she would remark after reading Dr. Conant's autobiography 20 years later, "and I never went to the Soviet Embassy for anything as cozy as a tea."[17]

Chapter 4. Choosing the Board and a Director

[1] New York, 1970. See pp. 64-66, 302-305.

[2] Personal interview.

[3] Penick, p. 115.

[4] Steelman, *Science and Public Policy*, Washington, D.C., 1947, vol. 1, p. 6.

[5] Phone interview.

[6] Personal interview with Bronk.

[7] NSB Min., 1st 10 meetings, supplemented by interviews with board members and with Steelman.

[8] Ibid.

[9] Stephen P. Strickland, *Politics, Science & Dread Disease: A Short History of the United States Medical Research Policy*, Cambridge Mass., 1972, pp. 19-29.

[10] Memo, Carey to Staats: Recommended Changes in Science Foundation Bills, Jan. 26, 1949, BOB 39.32 (E8-21/48.1), NA.

[11] *Cong. Rec.*, 81st Cong., 2d Sess., p. A6356.

[12] See note 10.

[13] Phone interview with Steelman.

[14] Ibid.

[15] Personal interviews: Miss Doris McCarn and Mrs. Mary Waterman.

[16] Conant, *My Several Lives*, p. 562.

[17] Interview.

Chapter 5

First Things First

Born June 4, 1892 in the New York mountainside town of Cornwall-on-Hudson, Alan Tower Waterman did most of his growing up in Northampton, Mass., where for 33 years his father, Frank Alan Waterman, was a professor of physics at Smith College. As the younger Waterman would say later, his decision at an early age to pursue his father's profession encountered "no resistance at home." Not that anyone would have been surprised had he set out instead to be a musician. He had perfect pitch and was adept on several instruments, the viola and the piano among them. Long after his retirement as Director of the National Science Foundation, oldtimers there would remember with a smile his practice on festive occasions, especially on those of interest to his Scottish forebears, of going from office to office, skirling the bagpipe.

After science and music, his greatest interest was the out-of-doors. During his undergraduate days at Princeton he picked up a summer job as a camp counsellor in the woods of Maine, earned his license as a Maine guide and made a point of renewing it annually throughout most of his life. After he became a father, a couple of weeks canoeing and fishing on the Allagash River in northern Maine was an almost yearly family ritual. As Waterman once remarked, he wanted his children to know those clear streams and deep-breathing forests as he knew them, not as they were fast becoming under increasing tourism and industrialization.

He took all of his degrees at Princeton: B.A. in 1913, M.A. in 1914, and Ph.D. in 1916, following an exploration of the positive electrical emission from hot solids under the guidance of the British Nobel laureate, Sir Owen W. Richardson. On then to the University of Cincinnati for his first teaching job. It was here he met Mary M. Mallon, a fair-haired 22-year-old assistant in the economics department. They were married in August 1917, would become the parents of three sons and two daughters, and in the fullness of time count grandchildren to the number of 17.

After Cincinnati came Yale. Waterman's long teaching career there was broken during the first World War by a 2-year tour of duty with the Army Signal Corps' Scientific and Research Division where he served from private to first lieutenant; in 1927 by several months as a National Research Council Fellow at King's College, London, where again he studied with Sir Owen Richardson; and 10 years after that by a leave of absence to teach for a year at Massachusetts Institute of Technology. He was an associate professor at Yale when in 1943 his good friend, Karl Compton, lured him to Washington.

As deputy chief of OSRD's Office of Field Service he traveled from one theater of war to another. *Newsweek* pictured his work during the second World War as "checking on United States weapons and cajoling dubious commanders to use new ones." According to the magazine, when "the British objected to developing microwaves to improve radar," on the grounds that the war would be over before it could be done, Waterman "tactfully" persuaded them to go ahead. Finding the Army and Navy in the Pacific feuding over what types of rockets to develop, he quietly made up their minds for them with the help of General Douglas MacArthur. *Newsweek* gave much of the credit for the efficient bombing by America's B-29's to the scientific analysts working under Waterman's direction.

After the war, as Chief Scientist in the Office of Naval Research, he endeared himself to America's academic scientists by emphasizing fundamental research. By his last year with the Navy, ONR was expending $40 million annually and subsidizing well over 40 percent of all of the country's basic science.

He was 58 when Truman picked him to head the National Science Foundation, a stocky, well set up man with a square and finely moulded face under a flap of silver hair. Not a large man, although Doris McCarn, his capable and attractive secretary, has remarked that "if you were to see Dr. Waterman only in a sitting position, you would not think of him as small." The law fixes the NSF Director's term at 6 years, and Waterman would hold the position under three Presidents. Eisenhower would reappoint him in 1957, and Kennedy would let him finish his second term by waiving the Government's compulsory retirement rule on the occasion of the Director's 70th birthday in 1962. Commenting on his quick confirmation by the Senate in March 1951, *Newsweek* described him as "a soft-spoken diplomat." A noteworthy observation in view of the traits he brought to his new labors: patience, firmness, modesty, and above all, prudence, a quality that some critics would later contend the first NSF Director sometimes carried to the point where virtue blended into fault.[1]

What he most needed in those starting months in 1951 was endurance. Extra hands, extra eyes, extra hours in the day, and an army of assistants ready to work around the clock would have helped too. In April the Director informed the editor of a scientific journal that all of the people at NSF "were going around in frantic circles. It is indeed something to try to start an agency which begins with one person trying to solve all pressing questions. . . ." But however frenetic the daily round, the Director somehow made time to record it. His chronological file with its numerous "diary notes," its paraphrases of office conferences and telephone calls, its touching comments on the unavoidable trivia of organizational birth, is a revealing record. Even if its subject were not the subject of this narrative, it would be of value as a picture of the myriad details involved in starting a Federal agency.

Waterman's official notice of his appointment came by telephone from the White House on March 9. That same day Conant and Bronk called on him at ONR, where for the next few weeks his office would also be National Science Foundation headquarters. Conant came as Chairman of the Board, Bronk as head of the Board's Executive Committee. The purpose of their visit was twofold, to apprise the new Director of what the members of NSB—the National Science Board—had done to date, and to discuss with him what must now be done to get NSF going.

The three men had much to talk over. Among other things, they discussed those clauses of the law that ordered the Foundation to develop a national science policy and evaluate the research programs of other Federal agencies. All three recognized that these sections of the enabling act spelled trouble. Neither Conant nor Bronk argued with Waterman's flat assertion that the Foundation should avoid "any actual evaluation" of the work of other agencies as "too dangerous and difficult" for a tiny newcomer to the United States Government's giant bureaucracy. Most of the other topics canvassed dealt with the mechanics of the Foundation's own programs or with the criteria to be followed in choosing members of the senior staff.

Waterman's summation of the session conjures up a long and earnest conversation among three unusual men: the Director himself, outwardly bland and reserved, but capable when his cherished predilections were challenged of digging in his heels; Conant, fleshless as an arrow in his person, sharp in his mind, one of the most influential educational leaders and thinkers of his era; Bronk, with his long face and bold features, brilliant, humorous, debonair, as much at home with practical men as with intellectuals, appreciative of the good things of this world, the scientist as bon vivant.

After Conant and Bronk left, Waterman drew up a list of those matters that the three men had concluded were "of primary interest"—the problems to be kept in mind as they went about shaping their organization.[2]

Before anything could be accomplished, a staff must be assembled. For Director and Board this meant fleshing out the structure called for by the law. Under the enabling act, the Director's senior staff would include a Deputy Director along with whatever assistant directors would be needed to run the agency's operational divisions. One division, to be known as MPE, would decide what programs and projects the Foundation would support in the mathematical, physical, and engineering sciences. Another—BMS—would do the same for biology and medical research.* Still another—SPE for Scientific Personnel and Education—would survey the scientific manpower situation in America, promote scientific education throughout the country, and oversee the granting of aid to college students preparing for scientific careers.

It was understood that the operating-division heads would be assisted by several program directors, each concerned with a specific field. MPE, for example, would have a program director for physics and astronomy. BMS would have one for molecular and genetic biology. In SPE there would be a director for fellowships and one in charge of gathering statistics on the country's scientific manpower needs. In accordance with the terms of the law, SPE would in time take over the National Roster of Scientific and Technical Personnel, then being compiled by the United States Office of Education.

So much for the top of the heap. The enabling act envisaged NSF headquarters as no more than the pinnacle of a pyramid whose large base would consist of a complex of advisory bodies and individual consultants. Each operating division was to have the assistance of a committee of experts. NSB had already provided a nucleus for these groups from its own membership. Waterman, with the Board's approval, now started to add others. Simultaneously he began setting up a network of panels whose members would advise the Foundation's program directors.

Underlying this arrangement was a principle that numerous witnesses had voiced at the legislative hearings in Congress: The determination of what research activities deserved Federal assistance should be made by men and women who were themselves competent in the sciences covered. Only a chemist could appraise the merits of a proposal by a Tennessee researcher to explore the *Reaction of Alpha, Beta-Unsaturated Ethers with Grignard Reagents.* Only a mathematician was in a position to pass judgment on the desire of an Indiana investigator to look into *Asymptotic Behavior and Stability Problems.* The day would come when NSF's many-tentacled advisory structure would have its critics. "Talk about interlocking directorates!" This, in essence, would be complainants' text; but grumbles of this sort would not arise until much later, in accordance with the well-understood dynamic, that before the renovators and the wreckers can do their thing, somebody must build the house.

---

* For a brief period at the beginning, there were two separate divisions, one for the biological sciences and one for the medical sciences. At an early date, the two were combined into BMS.

Waterman built it as fast as circumstances permitted. There were problems, some normal to the procedure, others special to the Foundation. One of them, he told physicist Gaylord Harnwell of the University of Pennsylvania, was that the talents required for getting an agency started were not necessarily the same as those that would be needed after it got going. Finding people with both capabilities wasn't all that easy. One had to "be very certain to get only the key staff which the Foundation will require for the future."[3]

Recruiting the right people for the operating divisions entailed an extensive knowledge of who knew what and who had done what in the country's scientific, educational and public affairs communities. Granted that the division heads and their program directors would have the help of knowledgeable advisors; in actual practice the final decision as to what research and educational activities merited Foundation support would usually be their responsibility.

The NSF law allowed the Director to hire top professional and technical people on an excepted basis, meaning without regard to Civil Service Commission pay scales. The Civil Service heads had already squawked about this, and in June, Waterman reacted defensively on learning that CSC and the Budget Bureau were sponsoring a study aimed at reducing the percentage of senior positions throughout the Federal bureaucracy.

Such a reduction at NSF, Waterman wrote CSC Chairman Robert Ramspeck,[4] would undermine the Foundation's role as the country's "scientific leader." In a letter marked by a scrappy tone found only occasionally in the Director's official communications, he reminded the Civil Service chief that the "key staff of the Foundation must be in close and frequent contact with the top . . . people of the universities, the endowed foundations, the industrial research laboratories, and the Government itself. They must be . . . people . . . universally recognized as the peers of those with whom they work, in professional attainment as well as in intellectual capacity." The Foundation, he stressed, needed an unusually large number of highly trained people to carry out its "responsibility for evaluation of the research program of the Government as a whole." In making this point, Waterman cannot be readily acquitted of deviousness in view of his recent and emphatic declaration to Conant and Bronk that the Foundation should sidestep its evaluating functions.

Nothing in the record suggests that NSF's privilege of paying some staff members above scale was at any time seriously threatened. One of Waterman's weapons in selecting a competent Deputy Director was his ability to offer $14,000 a year as against the $11,000 attached to the equivalent Civil Service rating. His choice for this post was Charles E. Sunderlin, Science Director of ONR's office in London. For Conant and Bronk, Waterman had outlined the qualifications he hoped for in his administrative alter ego. As he himself was a physicist, it followed that his Deputy should not be. What Waterman wanted was "an able administrator" with a scientific background, "preferably one familiar with Government operations."[5]

Tall, handsome Sunderlin fitted the measurements nicely. Holder of a doctorate from the University of Rochester, he had taught chemistry at the United States Naval Academy for several years before joining ONR-London in 1946. He had already decided to leave his post there when in early 1951 Waterman broached the possibility of his joining the Foundation. At that time two other jobs were looking for Sunderlin. The Department of State wanted him as its first Deputy Science Adviser. A major university in the Boston area had offered him a faculty appointment. He was weighing these possibilities when Conant, visiting London in April, called on him after a telephoned request to do so from Waterman.[6] Sunderlin would say later he took the NSF job because it meant working in an agency that was "trying something entirely new." Joining the Foundation in May, he would remain until 1957, resigning to take a fling at another "something entirely new," the directorship of "the first fundamental research laboratory ever set up by a major American corporation [Union Carbide] in Europe." Some years later he would return to the United States to become first a member of the staff of the National Academy of Sciences and then Vice President and Secretary of Rockefeller University in New York City.[7]

Waterman turned more than once to his old bailiwick, the Office of Naval Research. Even as he angled for Sunderlin's services, he was on the phone and writing letters in an effort to wean Harry C. Kelly away from his job as head of the science section of ONR-Chicago. Kelly had earned his doctorate at MIT, had worked there as a researcher during World War II at the Radiation Laboratory, one of the larger installations funded by Vannevar Bush's OSRD.

Early in 1951 Waterman was listening to a plaintive plea from Joseph B. Koepfli, the new science adviser to the State Department. Koepfli said he was "staking a claim on Kelly." As he had recently "lost Sunderlin" to NSF, he trusted "this would be agreeable to Waterman." The Director was compelled to tell him that Kelly had already expressed a preference for NSF. Waterman was all sympathy and politeness, but the diary note[8] covering his chat with Koepfli exudes a feeling of pride that for the second time puny NSF had snatched a valuable man from powerful State. In May, physicist Kelly took over as head of the Division of Scientific Personnel and Education. Later he would discharge other duties, leaving NSF in 1962 to become first

Dean of Faculty and then Provost at North Carolina State University in Raleigh.

Several other ONR men answered the call. One was John Field, although by the time Field took over as head of the Biological and Medical Division, he was no longer with ONR. He had accepted an appointment as Chairman of the physiology department of the University of California School of Medicine in Los Angeles, and would spend his limited time at the Foundation on a leave of absence from that institution.

Other experts also signed up on a leave-of-absence basis—Paul E. Klopsteg of Northwestern University, for one, as the first head of the physical, mathematical, and engineering sciences division. In the early days Director and Board looked kindly on this arrangement as a means of opening channels between NSF and the schools. It is still in use, especially where program directors are concerned.

The charter staff included several women. Joining the Foundation in 1951, Lee Anna Embrey (now Mrs. Roy E. Blick of Washington) was for several years Waterman's chief researcher, assisting him in the preparation of speeches, papers, and magazine articles. Virginia B. Sides was another recruit from ONR-London. As a young Wellesley graduate in 1944, "Ginny" had begun her adult career as a junior executive at OSRD and had later moved with Dr. Waterman to the Office of Naval Research. Her coming to the Foundation in late 1951 to work in the educational division was in the nature of a family reunion.

Although the Director's wife was averse to mixing home and office, she made a contribution to the Foundation. Soon after her husband's appointment to the Office of Naval Research, a journey to upstate New York had given Mrs. Waterman an idea. The purpose of the trip was to see a newly acquired daughter-in-law. She found the young woman in tears and saying bitterly that "marriage isn't what I thought it was going to be at all." Her plaint grew out of an old American phenomenon. She and her husband were newcomers in town, and so far none of the natives had come to call.

Enroute home, Mary Waterman found herself thinking about the scientists coming from all over the country to work for her husband. Back in Washington she organized a club for the wives of all ONR employees. When Dr. Waterman went to the National Science Foundation, she did the same there. Nearly every month, for 12 years, her white-brick house in the Westmoreland Hills section of the city was the scene of these gatherings.[9]

Mrs. Waterman was popular with the women on her husband's staff. To Ginny Sides she was "of all the characters in the world . . . one of the most interesting . . . a delightful woman." What most

86

struck Miss Sides was that Mrs. Waterman never appeared to be awed by "the pomp and circumstance" of Washington. "It was in the ONR days," Ginny would recall in later years, "that Dr. Waterman was being given . . . a medal for his wartime services and was going off to the office of the Secretary of Defense. Mrs. Waterman came into our office with a little tiny black toque that she wore for everything. But on that particular spring morning she had taken some English ivy out of the garden and wrapped it around her hat. That was Mrs. Waterman's way of dressing up."[10]

By early 1952 the senior staff of the Foundation as then planned was complete. According to a report drafted on February 20 by the first General Counsel of NSF, William A. W. Krebs, Jr., the total roster, exclusive of members of the Board, came to 22 scientists serving parttime as Divisional Committee members, 20 other part-time employees, and 62 fulltime employees. Already the Foundation had outgrown its first home, an old 17-room townhouse at the corner of Washington's 16th and Eye Streets Northwest. Its second home, a onetime schoolhouse at 2144 California Street Northwest, had become overpopulated. Dr. Waterman would soon be telling William E. Reynolds, Commissioner of Public Buildings Service, that he hoped the Foundation's next home would not only be large enough but that it would also have the dignity appropriate to an agency whose visitors' list regularly included presidents of universities, industrial leaders, and distinguished foreign scientists.

All told, NSF headquarters would have four different addresses before arriving in April 1965 at its present accommodations in a big, beige-colored, 12-story office building two blocks west of the White House in downtown Washington. Neither of its first two homes was notable for elegance. A feature of the "three-story and basement dwelling" on 16th Street was the presence in the cellar of a high colonic irrigation apparatus, left there by a previous tenant. Waterman, speaking in 1951 before the National Association of State Universities in Chicago, described this house as "not quite old enough to be antique and not quite young enough to boast any modern appurtenances. Nevertheless it has a certain modest charm of its own, perhaps typical of the modest role the Foundation is prepared to play at the present time." At the second home, the bigger four-story building on California Street, the roof leaked and the entrance steps were so rickety that most employees came and went by the fire escape.

The Foundation's third home, occupied in June 1953, enjoyed at least a certain historical eclat. It stood on the corner of H Street and Madison Place, across from famous Lafayette Square. Known locally as the "Old Cosmos Club," it actually consisted of four houses. In one of them Dolley Madison had lived for several years after leaving the White House. Arguments over which office was in Dolley's bedroom were still being heard when in August 1958 NSF moved again, this time to the old concrete home of the Public Health Service on Constitution Avenue Northwest, now well known, if not indeed faintly notorious, as the home of the Bureau of Indian Affairs.

That the NSF people were determined to keep their early homes neat and tidy is attested to by numerous entries in the older Administrative Services files. "Office Services Instruction No. 16 (Revised)" for June 29, 1960, for example, made two of the employees responsible for watering the flowers in the lobby. "Once a week," the instruction read, "the leaves are to be washed off to prevent an accumulation of dust which not only spoils the appearance of the plants, but also interferes with their breathing. The two rubber plants on the steps are to be turned daily to keep them from becoming one-sided."

During Director Waterman's opening month in office, making the arrangements for moving into the 16th Street house consumed a good deal of his time. With the aid of the Assistant Director for Administration, Wilson E. Harwood, he saw to it that phones were installed and a cleaning crew employed along with guards for the building. He wrote the local phone company, asking that the Foundation be listed in the Directory under "N" as well as under "United States Government." The minute all was ready he invited Board members to an open house at the Foundation's first headquarters on the occasion of their next meeting in the city on May 18, 1951.

The party was nonalcoholic. The guests, arriving in late afternoon, made do with less stimulating beverages. But nothing could sober the festivities. Board member Joseph C. Morris, vice president of Tulane University, got a knowing laugh when he lofted a pitcher of tomato juice and asked, "Is this the blood we sweated out of Congress?" To make certain there was coffee enough, the women of the staff had brought in their electric urns. Too many of them, it would seem. Suddenly, as the party was nearing an end, all of the fuses blew and the recently installed sprinkler system began to squirt. With the arrival a few minutes later of a D.C. fire wagon, the National Science Foundation's first social affair came to a clangorous conclusion.[11]

During those starting months in 1951 all of the work of the new agency was done on a hope and a prayer. The hope was that for fiscal year 1952 (beginning July 1, 1951) Congress would allocate sufficient funds to permit the Foundation to begin actual operations in support of basic research and education in the sciences. Waterman told the Budget Bureau that he thought $14.75 million would be about right. As that sum nudged the $15 million ceiling then stipulated by law, the Bureau decided that a slightly more modest request would stand a better chance in Congress. When on May 21 Truman sent a billion dollar supplemental appropriations bill up the Hill, he included a $14 million request for the Foundation.

House subcommittee hearings on the NSF section of the bill fell on June 6. Waterman, Bronk and other Board members were present to testify. Waterman stressed the importance of basic research. He dipped into history to show how much mankind has benefited from the efforts of great explorers along the frontiers of science. Among others, he pointed to England's Edward Jenner, whose investigations had produced a vaccine for smallpox; to Italy's Marconi, whose study of electromagnetic waves had led to wireless telegraphy. As the Director finished, a *Washington Star* reporter detected a bewildered expression on the faces of some of the committee members. Republican Congressman John Phillips of California found the Director's remarks "informative," but couldn't quite see their relevance to the problem at hand. Phillips didn't claim to be an expert on the subject. Still it was his understanding that none of the scientific giants mentioned by Waterman had done their work on Government subsidies. "Take Jenner," he said. "You would not have given Jenner a scholarship. He was completely opposed by the whole medical fraternity of his day. If you had given him a nickel the medical profession woud have been down to see their respective Congressmen the next day."

Sensing a little nervousness among the Foundation people at these anachronisms, the Subcommittee chairman, Democratic Congressman Albert Thomas of Texas, hastened to reassure them. "You will certainly make a success of this [NSF]," he said, "and this committee is going to do its best to cooperate with you and help you." Waterman and his colleagues left the hearing room hopeful, but far from confident.

Anxious weeks ensued. The opening of the new fiscal year (July 1) came and went. On July 9 Board member Charles Dollard, former president of the Carnegie Corporation of New York, phoned Waterman. Any news? None, he was told. In the absence of an appropriation, the Foundation "was carrying on on an extension of the administrative budget" of the preceding fiscal year.

On August 17 the suspense ended. The House Appropriations Committee reported the supplemental funding bill. Truman's larger requests, most of them having to do with the stockpiling of materials for national defense, came through with little damage. As for the Foundation—the Committee recommended and the House shortly voted an FY 1952 budget of $300,000.

A 98 percent cut! During its quarter of a century NSF has weathered other setbacks. None of them, however, has jarred its leaders or distressed its friends as thoroughly as this one did.

The news broke on a Friday. By mail, by wire, by phone, Waterman began at once calling on the friends of the Foundation to rally to its defense. One of his first moves was to issue a statement to the press. On Saturday copies of it went to the 24 Board members and to some 40 other individuals, all of them holding crucial positions in Government or industry, or in the scientific, public affairs, and academic worlds. The gist of his mimeographed letter to the NSB people was that all was not lost. The Senate had not yet acted. The Foundation would have another chance to present its case when the Senate Appropriations Committee, chairmanned by Kenneth D. McKellar of Tennessee, got around to considering the NSF section of the supplemental funding bill.

"You should know," Waterman told the Board members, "that it is not uncommon for the House to be more severe on appropriations than the Senate." In a letter to McKellar, he spoke of arrangements under way for having members of the Foundation staff and the Board testify at the forthcoming Senate appropriation hearings, along with three or four other "distinguished persons having an intimate knowledge of the Nation's needs for basic research and education in the sciences."

Conant was traveling in the South Pacific. Waterman caught him by phone in New Zealand. The NSB Chairman would be heading homeward in a few days. If he got back in time, he would testify at the hearings. Bronk, reached in New York City by telephone, said he would get in touch immediately with Senator Salstonstall. He vetoed a suggestion that the Board hold an emergency session. All the members could do was pass a resolution. Bronk didn't think the Senate would be much impressed by a statement from the National Science Board advocating a good budget for the National Science Foundation. At Tulane University in New Orleans, Board member Joe Morris made plans to stop off in Washington during an already scheduled trip north to talk with Senator Allen J. Ellender of Louisiana.

Waterman's widely distributed pleas for help brought messages of support from several industrial leaders. In a letter designed for transmittal to the Senate Appropriations Committee, Standard Oil board chairman Robert E. Wilson stressed the importance of the Foundation's work to industrial progress. International Latex Corporation ran newspaper advertisements in the *New York Times* and the *New York Herald-Tribune*, asking Congress to give the Foundation a working budget.

The *Times* and the *Washington Post* deplored the House cut. "Sound economy," the *Post* editorialized, "embraces the conservation and cultivation of national resources. In the light of this standard, the recent action of the House in slashing the appropriation request of the National Science Foundation must be viewed as a piece of pennywise foolishness." The *Post* said the "pittance" approved by the House "precludes research and training alike, leaving the agency without any real function. This is not economy; it is wreckage—and wreckage of a program vital to national welfare and national defense."

The *Post* titled its editorial "Killing the Goose." In another issue Herblock expressed the idea in pictorial terms. His cartoon depicted Congress in the person of a rotund gentleman on the verge of putting the ax to a goose called "Scientific Research." At his elbow an agitated Uncle Sam pointed to a nest filled with eggs that the doomed goose had been trying to hatch. The eggs were labeled "Security Devices," "Miracle Drugs," and "New Weapons." Uncle Sam was quoted as saying to Congress, "Let you in on a secret. There's a connection here."

By the first day of September Conant had returned to Cambridge and Waterman was writing him a long letter "by way of a preliminary 'State of the Union Message'." Many friends of the Foundation had written to Senator McKellar. Waterman listed, among others, Vannevar Bush, Senator Herbert Lehman of New York, the uniformed heads of the Office of Naval Research, and the general manager of the Atomic Energy Commission. John R. Steelman had written for the President. After several postponements the Senate appropriations hearings were set for the fifth. "Our general impression," Waterman wrote, "is that the Senate will certainly do something about our predicament."

It did. The committee recommended and the Senate voted $6.3 million. Only one hurdle remained. Conferees from the two branches of Congress had to meet to reconcile variations in the House and Senate bills. The final sum approved by Congress on October 22—$3.5 million—was a measure of the ability of the American scientific community to spring to arms for its own interests, of the stature of quiet, unpushy Alan Waterman as its leader.

One battle won. Another already in process. From the beginning the Foundation managers had regarded the $15 million budget ceiling in the enabling act as a bar to the fulfillment of its missions over the long pull. In November 1951 they persuaded the Budget Bureau to back them up in an effort to have the ceiling lifted. On June 3, 1953, their old friend in the Senate, H. Alexander Smith of New Jersey, introduced the necessary legislation. Two months later—on August 4--the ceiling was gone.[12]

Congress had assigned a dozen or so activities to the Foundation, but essentially all of them fell into two large categories. One category, as has been mentioned, involved whatever steps NSF was able to take toward the development of a national science policy. The other embraced the agency's operational chores—its efforts to promote basic research and education in the sciences.

The Foundation undertook to fulfill its policymaking obligations by gathering data and publishing surveys concerning the strengths and weaknesses of the country's scientific and engineering work and the extent and distribution of its scientific and technical manpower. In Waterman's view this was as far as the Foundation should go as a policymaker. He was adamantly opposed to any procedures that might tend to make him a czar of science or put his agency in the position of dictating to other Federal agencies.

His views on this matter were not welcomed at the Bureau of the Budget. Very quickly indeed, as will be brought out more fully in the next chapter, a breach developed between the Director's concepts of national policy on the one hand and those held by interested officials at the Bureau on the other. Waterman saw the creation of national policy as an evolutionary process. His contention was that such a policy would gradually emerge from the scientific community itself as, little by little, the surveys sponsored by NSF illuminated the needs and potentialities of the Government's research and development programs. To the Bureau officials this was too narrow a concept. They wanted the Foundation to play a broader role by continuously assisting them in their efforts to distribute available funds in the manner most likely to hold duplications of scientific work to a minimum and to expose gaps in the Federal program that should be filled.

No strains of this sort marked the efforts of the Foundation to assume its operational duties. In these departments it moved ahead from the start with impressive decisiveness and with the zeal to be expected of an organization run by a man as convinced of the value of pure science and as sensitive to the climate needed for its cultivation as Alan Waterman.

During the summer and fall of 1951 the Director and his aides tackled a problem that had to be solved before research support activities could begin in earnest. By what methods should the agency finance scientific work in the universities and elsewhere? At the Office of Naval Research, Waterman had done this by contract. Not, however, from choice but from necessity. Under the law as it then stood, the Navy could fund research outside of its own laboratories in no other manner. Except for the Public Health Service, which could issue grants, the same rule applied to all of the other Federal agencies except the newly funded NSF.*

---

* Largely at the instigation of NSF, Congress in 1958 extended the privilege of using grants to finance extramural scientific work to all Government agencies.

No restrictions of this sort bound the Foundation. It could support research in any way it wished. The question then was what system would work best. Should it lend money to scientists or set up prizes for those doing exceptional work? Should it rely principally on contracts or on grants? Staff members examined the devices employed by some of the country's private foundations. They discussed the problem with the grant-making officers of the National Institutes of Health and solicited the opinions of scientists in universities around the country. In July, Deputy Director Sunderlin wrote Ginny Sides, then still in London, asking her to look into the procedures of the University Grants Committee, a quasi-governmental agency charged with obtaining financial help for England's institutions of higher learning.

A late-July position paper,[13] reflecting a staff consensus identified six known "techniques of fostering research," discussed them pro and con, and offered recommendations for consideration by the Board. Under the heading "Grants," the staff listed several types. It spoke of the "block" or "fluid" grant, under which NSF would give a sum of money to an institution whose authorities would then select the kind of work to be done and the people to do it. Mentioned also was the "project grant." Under this arrangement the Foundation, having approved of a proposal, would send the money to an institution for use by the "principal investigator," meaning the scientist who had planned and suggested the project.

The staff foresaw conditions under which the Foundation might find use for any of the research support techniques then known. It hoped that in addition the agency would develop new ones as "the need therefore" arose. It asserted that under no circumstances should NSF limit its choice of methods "by administrative action."

The staff's major "judgment" was that in most instances the Foundation should use the project grant. Whereas a contract implied a "specific piece of work to be done," a grant implied that the work be general in nature. "For the most part," the position paper pointed out, "work supported by the Foundation will fall in the latter category." A contract, according to the staff, "implies a close control over the work contracted for, while the grant implies a freedom on the part of the investigator to pursue his work subject to broad conditions." The staff conceded that in actual practice grants could be more restrictive than contracts. Inquiries, however, had brought out that most scientists felt freer working under a grant than under a more formal arrangement. Another staff argument was that grants were simpler to handle than contracts. "Having fewer conditions and accountability requirements," the position paper stated, "they can be negotiated more quickly . . . ; and there is less need for a detailed follow-up in the form of a financial audit or formal evaluation of results."

In a long summation the staff suggested the terms under which grants should be issued. They should cover varying lengths of time, ranging as a rule from 2 to 5 years, depending on the nature of the research. Payments should be in advance, in a lump sum or in increments on preset dates. The institution where the grantee did his work should receive a sum equal to a specified percentage of the amount of the grant to cover expenses connected with its administration, overhead, and other indirect costs. As a rule, all equipment purchased with NSF funds should remain the property of the grantee institution. The staff admitted that this procedure would be "in sharp distinction to the general practice under Government contracts where the Government retains title." It quoted both Government contractors and agencies, however, as saying that "because of the specialized nature of research equipment . . . and its high obsolescence," the costs of the recordkeeping necessary when the Government kept title would almost always exceed the value of the facilities. The staff contended that the Foundation had an "additional justification" for permitting retention of equipment in that such a practice would help colleges and universities to improve their laboratories and their science training programs.

Other recommended procedures dealt with patents and with the policy to be followed when a research project cost less than the amount of the award. The staff noted that private foundations usually left the question of patent to the grantee. Because of possible "political repercussions," however, there was a feeling that the Foundation should not "be quite so liberal." The staff advised that when a grantee filed for a patent he notify NSF and give the Government a royalty-free license. Unused funds, in the view of the authors of the position paper, ought to be returned except when the grant was about to be renewed. In that case, leftover monies should be applied to the extended award

Board endorsement of most of these recommendations was completed by late fall and during the first week in December the staff used mailing lists supplied by the American Association for the Advancement of Science and the American Council on Education to circularize the country's colleges and universities and other nonprofit research centers, inviting them to submit proposals and outlining procedures for doing so. On February 2, 1952, the Foundation issued its first 28 grants. The largest of them—$50,000—went to the University of California, Berkeley, to enable two biologists to conduct a 5-year experiment in genetics. The smallest—$780—was for the University of Minnesota. In underwrote a 2-year study by a faculty botanist of "Coal Ball Floras," the microscopic plant remains found in the nodules of coal.

Even before its first 28 grants went out, the Foundation had completed plans for the first of what later became a complex of programs aimed at increasing the country's supply of trained scientists and engineers. The urgency that the Foundation attached to this effort reflected the labor situation in the 1950's. Discussing the problem in its second Annual Report in November 1952, the Foundation reproduced recent headlines from the *New York Times:* "Government Seeks Scientists," "Engineers Scarce in Plane Industry," "Skilled Scientific Manpower: one of Nation's Great Needs." The Annual Report quoted M. H. Trytten, director of the National Research Council's Office of Scientific Personnel, as saying that at one meeting of his organization in 1951, representatives of 16 major employers of scientific labor had announced that "after scouting the Nation's graduating classes they were able to obtain on the average only 36 percent of the new employees needed."

At several early sessions the National Science Board considered making both scholarships and fellowships accessible to students of science in the country's colleges and universities. The scholarships would go to undergraduates, the fellowships to scholars working at higher academic levels.

Two reasons underlay the decision to offer fellowships only. One was that in the early 1950's the United States Office of Education had indicated its intention of starting an extensive scholarship program. The other reason, less publicized but equally persuasive, was a fear that many undergraduates with little or no real interest in science would apply for NSF assistance simply to underwrite their college educations. It was the opinion of Harry Kelly, head of NSF's educational directorate, that to encourage this practice would be "immoral." It would be tantamount, Kelly said, "to saying to a high-school senior, 'If you will go into science, the Foundation will give you a college scholarship.' "[14] The National Science Board endorsed Kelly's view, probably because most of its members were research-oriented and happy at the prospect of spending on grants for basic research the money that a scholarship program would have consumed.

The Foundation's fellowship program, as approved by the Board, was not limited to predoctoral graduate students. Awards were also made available to holders of the Ph. D. degree interested in devoting a year or more to fulltime research. The thinking responsible for this aspect of the program went back to Vannevar Bush's *Science, The Endless Frontier.* A recommendation from one of Bush's advisory groups, a committee headed by Isaiah Bowman, was that the Foundation sponsor a few postdoctoral fellowships as a "direct aid" to research. "Research workers who have reached the status of assistant professor or above," the Bowman committee argued, 'tend to remain in their own universities and their time available for research tends to become increasingly broken up. . . . The resulting immobility of the senior staff serves to isolate the intellectual life of a university from that of its fellows, and the individual concerned, lacking outside stimulation, may incline more and more to perfunctory performance of routine duties. . . . Fellowships large enough to meet the salaries of advanced academic personnel for periods of intensive research work . . . would be an effective means of attacking these problems."[15]

On the intent and nature of the Foundation's first fellowships, mild-mannered Dr. Waterman harbored strong opinions. He made this clear during a May 1951 phone talk with Frederick C. Schuldt of the Bureau of the Budget.[16] Schuldt said that there seemed to be thousands of students of science in the country's graduate schools.* Why then was NSF talking about a far fewer number of fellowships? Didn't it want to support them all? Waterman replied that it most certainly did not. To support them all would rob the NSF fellowships of prestige. In many cases, moreover, it would put the Government in the indefensible position of underwriting incompetents.

Should a means test be used? The enabling act specified that fellows be selected on the basis of ability. There was nothing in this provision, however, to prevent NSF from giving poor students more money than rich ones. Waterman dismissed suggestions to this end out of hand. Since nearly all of the applicants would be in their early 20's, most of them could be assumed to be "without means." Granted some would have richer or poorer parents, but the fellowships were not intended for parents. The National Science Board went along with the Director's thinking. In July 1951, by formal resolution, it forbade the use of a means test either in the choice of fellows or in the determination of their stipends.[17]

In August, under the first contract negotiated by the Foundation, Dr. Trytten's Office of Science Personnel, a branch of the National Academy of Sciences' National Research Council, set up mechanisms for the evaluation of candidates.[18] Each applicant had to submit a record of his grades, the test scores he had received on certain scientific aptitude and achievement examinations, and the recommendations of faculty advisers and others acquainted with his scientific abilities. To assist in picking the first NSF fellows, 106 experts, representing all aspects of science, sifted the applications. This done, Dr. Trytten's office sent the names to the Foundation in rated batches. Quality Group I consisted of applicants with such high ratings that all were regarded as eligible. Although the NSF law made ability the sole criterion, it stipulated that in choosing between candidates of equal ability the Foundation should aim at wide geographical distribution. Consequently, the lower rated groups sent to the education division at NSF suggested geographic quotas for the States from which their applications came.

---

* According to information then available the number of graduate students in the natural sciences in 1951 was close to 50,000. (Francis G. O'Brien, memo to author, Dec. 9, 1974.)

By April 1952 the experts employed by the National Research Council had screened 3,000 applications, and the National Science Board had approved the issuance of 1,050 awards covering the forthcoming academic year. Of the 624 actually awarded, 569 went to students working toward the Ph. D. degree, the other 55 to those who already had it. Most of the finalists—85 percent—were from Quality Group I, and stipends ranged from $1,400 for first-year graduate students to $3,400 each for successful postdoctoral candidates.[19]

With its first grants and fellowships en route to their recipients, the Foundation's operating functions were well under way. In another department—its policymaking division—matters were not proceeding so smoothly.

Chapter 5. First Things First

[1] Sketch of Waterman based largely on *Chemical and Engineering News*, Mar. 26, 1951, p. 1212; ibid., Dec. 11, 1967, p. 20; *Newsweek*, April. 2, 1951; *New York Times*, Dec. 16, 1957; Records of Smith College; interviews with Mrs. Waterman.

[2] Diary Note Mar. 9, 1951. Chron File.

[3] ATW to Harnwell, Apr. 18, 1951. Chron File.

[4] June 6, 1951, Chron File.

[5] See note 2.

[6] Diary Note Apr. 10, 1951. Chron File.

[7] Personal interview with Sunderlin.

[8] April 12, 1951, Chron File.

[9] Interviews.

[10] Personal interview.

[11] Various diary notes and letters in Chron File and the Administrative Services files describe NSF homes and housekeeping problems. Old timers provided a few extra tidbits and the details concerning the first social event are from Miss McCarn and Mrs. Waterman.

[12] Coverage of the budget crisis of 1951 is based largely on Chron File. For progress of the supplemental appropriations bill for FY 1952, see White House press release of May 21, 1951, and *Cong. Rec.*, 82d Cong., 1st Sess., pp. 10396, 12777-80, 12782-83, A5315, A5505, A5631. For ATW's testimony and the remarks of Congressmen Phillips and Thomas, see editorial, "The Science of Appropriating," *Washington Star*, Ag. 21, 1951, and the *Bulletin of the Atomic Scientists*, Oct. 1951, p. 291. The *Washington Post* editorial quoted was in the Sept. 17, 1951 issue, and Herblock's cartoon appeared on Sept. 30.

[13] "Working Paper on Techniques of Fostering Research," NSF Historical Files, NSF Library. For a summary of it, see National Academy of Sciences-National Research Council, *Federal Support of Basic Research in Institutions of Higher Learning*, Washington, D.C., 1964, pp. 47-48. See also Lee Anna Embrey, "The National Science Foundation," *The Graduate Journal*, Winter 1963, pp. 301-318.

[14] Interviews with NSF officials.

[15] Diary Note May 9, Chron File.

[16] Diary Note July 5, 1951, Chron File.

[17] NSB Min. 7:12.

[18] Memo to NSB, July 6, 1951, Chron File.

[19] Diary Note Feb. 29, 1952, Chron File; ATW to E.B. Fred, Mar. 11, 1952, ibid.; Interviews with NSF officials; A.R. 1952, pp. 22-31.

Chapter 6

To Coordinate or Not to Coordinate

On the afternoon of May 15, 1952 William D. Carey of the Bureau of the Budget* traveled from his office near the White House in Washington, D.C., to the one-time schoolhouse that the National Science Foundation had recently taken over on California Street Northwest.

It was Mr. Carey's first look at the Foundation's new quarters and his dark, deep-set eyes missed little. What he saw was a "rambling old edifice with a ripe brick exterior and a kind of mouldy antiquity which subtly implies that administrations will come and go, receiving scant attention from the intellectuals within." Inside the building the atmosphere struck him as "peculiarly congenial to cigar smoke and good rich talk . . . an atmosphere of unhurried deliberativeness, and good if not fashionable living. It is an excellent residence for the dean of studies. And this is how the Foundation is being run."[1]

The purpose of Carey's visit was to try to persuade Director Waterman to run his organization along slightly less academic lines. A brilliant and long-time student of science-Government relationships, Carey was deeply troubled by the position that Waterman had taken *vis a vis* those sections of the NSF enabling act that required the Foundation to help develop national science policy and to "evaluate and correlate" the research activities of other Federal agencies. Repeatedly, Carey had urged the NSF Director to get on with his policy and coordinating chores. Just as repeatedly—to use Carey's never colorless language—Waterman had "ducked out of sight under the table."

In Carey's pocket on that spring afternoon in 1952 was the draft of a letter that he proposed to have sent to the Foundation by the Director of the Budget. Never mailed, the letter seems to have disappeared, and 23 years later Carey—now vice president of Arthur

---

* Now Executive Secretary of the American Association for the Advancement of Science.

D. Little, Inc., a research organization with offices in Cambridge and Washington—can reconstruct its contents only in general terms.

"I assume," he says, "that it expressed our desire at that time to make NSF a part of the budget review and rationing process. In essence the letter said to the Foundation, 'We need your advice. You know scientific research from the inside. We know it only tangentially. Please make the necessary assessments of the other agencies, so that you can tell us the scientific merits of the many and conflicting claims for research and development money that the Government receives from them.' "[2]

As Carey mounted the stairs to Waterman's comfortably furnished office, his hopes were low. He had reason to fear that the NSF Director would not take kindly to his proposed letter. The Budget Bureau man had been looking in on the Foundation intermittently ever since its inception. He and Waterman had had a number of encounters—enough for each to savor the other's thinking.

Carey's view was that he and his colleagues at the Bureau were "inviting the National Science Foundation into the parlor." He thought then and would still contend 23 years later that "had Waterman gone along with us, we could have put the National Science Foundation at the center of power and decisionmaking in Washington. Subsequently as you know, a number of arrangements were effected at the White House in an effort to implement the coordinating and policy obligations that the Foundation had so agilely sidestepped. As you also know, the most significant of these arrangements—the Office of Science and Technology—has of late been dismantled and those difficult policy-formulating and coordination functions have to some extent gone back to the National Science Foundation Director."

In the early 1950's, then, Carey was daring Waterman and his agency to climb to high places—and Waterman was responding in the spirit of the Biblical, "Get thee behind me, Satan."

As has already been noted, the NSF Director's concept of his job as the guiding spirit of the Foundation during its formative years left no room for the "power and decisionmaking" roles that Carey wanted him to assume. In Waterman's opinion his first and all-important task was to create an enduring mechanism for the support of fundamental science—and particularly for what he was fond of calling "non-programmatic" research, meaning that untethered exploration of the unknown that the other Federal agencies, limited to research related to their statutory goals, could not as a rule pursue. In his determination to avoid exercising the evaluating functions written into the NSF law, he was driven by the fear that any attempt by his "small, semi-invisible" Foundation to judge the research procedures of bureaucratic giants like the Department of Defense and the National

Institutes of Health would quickly dissipate its capacity for doing anything for basic science at all.

Waterman's long battle with Carey and the Budget Bureau—an illuminating episode in the growth of the Foundation—was untouched by rancor at least on Carey's side. "I salute Waterman" the one-time budget officer would say long after the two men had had their last argument. "He frustrated me at every turn, but he was magnificent!" and the ring in Bill Carey's normally low and unhurried voice clearly identifies his remark as an accolade to a worthy foe.

Their conference on California Street in May 1952 ran pretty much the course that Carey had anticipated. Waterman read the letter and asked why the Bureau wanted to send such a communication "now."

Carey listed the Bureau's reasons. The budget people thought "that NSF's hand needed strengthening in its dealings with the established agencies of research." They wanted "specific results from the NSF for use in connection with the autumn budget review." They "felt it would be well to recapitulate the needs of the Executive Office for assistance from the Foundation."

Detecting signals of dissent from Waterman, Carey hastened to add that "the Bureau would not cram a letter of this kind down his throat." He then reiterated his conviction that "it would be well to have such a document in the files of the Foundation, even though it might not be necessary for the agency to go around brandishing it as a search warrant."

Waterman described himself as "a little afraid that such a letter would hurt instead of help the Foundation." His reason for this unease was that representatives of the NSF and of the Research and Development Board of the Defense Department were already engaged "in joint planning activities relating to a division of responsibilities in basic research." He implied that the dispatch of the Bureau's letter might inhibit this fragile experiment in interagency cooperation. He assured his visitor, however, that he would take the letter "under advisement," and on this note Carey departed, unhappily, convinced that one more effort to needle the Foundation into discharging its coordinating and policy-forming duties had failed.

"Waterman's idea," he reported to his superior at the Bureau that afternoon, "is to operate on the basis of a scientist-to-scientist approach, instead of the agency-to-agency pattern. Out of this spiritual communion will emerge gentlemen's agreements leading to the formation of spheres of influence for research sponsorship. This philosophy shows itself every time we hear Waterman talk, and I have about decided that there is no use trying to change him."

The issues responsible for Carey's call on Waterman in May 1952 were rooted in some of the events connected with the origins of the National Science Foundation. The investigations underlying the Steelman Report of 1947 showed that it was not uncommon for four or five Federal agencies to be either doing or supporting much the same kind of scientific work.

"This duplication of effort prevailed," Steelman has said, "because where research and development were concerned no agency ever told another what it was up to."[3]

President Truman declared the situation "wasteful," and in December 1947 he followed through on one of the Steelman Report suggestions by creating the Interdepartmental Committee on Scientific Research and Development (ICSRD).[4]

It is a comment on the baffling ways of Government that although ICSRD was set up to oversee the Government's scientific undertakings, it was ill-equipped to do so. It was an army without ammunition. The executive order calling it into being did not require the heads of the departments and agencies engaged in research to serve on the committee. Instead the order permitted the agency heads to designate some "officer or employee" to represent them. As a result, as Bill Carey saw it, ICSRD was "bottom heavy." It consisted mainly of career bureaucrats, the last people in the world to want to breach the walls of silence that prevented one agency from knowing what the other agencies were doing. The committee members could carry on studies, draft reports, and offer recommendations. But they had no reliable access to the presidential ear. Consequently, as Carey has said, all they could do was "talk to themselves." The Interdepartmental Committee, he adds, "had no capability or any authority or any prestige to deal with matters of program or priorities or policy. . . . To be sure, it had observers from such places as the Bureau of the Budget, but it had no effective reporting channel. Nothing much was expected of ICSRD and they delivered nothing."

Convinced that an interagency body was not the answer to the coordination problem, the strategists at the Bureau of the Budget began casting covetous glances at the National Science Foundation bills then inching through the legislative maze on Capitol Hill. It will be recalled that after Truman vetoed the Smith bill in 1947, considerable time elapsed before both houses of Congress got around to acting on a substitute certain to elicit presidential approval. Not until March 18, 1949, did the Senate pass S. 247, the measure that as amended by the House would become the NSF enabling act. Not until June of that year did a House subcommittee under Democratic Congressman Priest of Tennessee complete its consideration of the almost identical measure, H.R. 4846.

In early 1950, neither S. 247 nor H.R. 4846 assigned to the forthcoming NSF any responsibility for the research efforts of other agencies. On the contrary, both bills ordered the National Science Foundation to stay in its own yard. "The activities of the Foundation," said section 14(h) of the House bill, "shall be construed as supplementing and not superseding, curtailing or limiting any of the functions or activities of other Government agencies engaged in scientific research and development."[5]

To most of the scientist-members of ICSRD these were comforting words. They believed that the National Science Foundation could best serve the Nation by first of all serving science. That objective, in their opinion, could never be reached if the Foundation had to police the work of the other agencies.

The Budget Bureau strategists marched to a different drummer. In their eyes the Foundation could operate in the national interest only to the extent that it served the President. Sometime during the summer of 1949, Elmer Boyd Staats, then executive assistant director of the Bureau, discussed his agency's views with members of the subcommittee considering the House NSF bill. He told them that the National Science Foundation looked like the proper place in which to lodge the science coordinating functions that the ICSRD had neglected to exercise.[6]

Staats found a sympathetic listener in Chairman Priest of the subcommittee. He found another in the ranking minority member, Republican Congressman John Walter Heselton of Massachusetts. As Stephen P. Strickland observes in his entertaining study of United States medical research policy, *Politics, Science, & Dread Disease,* any time you can line up for your cause both the chairman of a Congressional subcommittee and the top member of the opposite party, you have an "all-powerful combination" behind it. So matters worked out for Staats. Long before H.R. 4846 left committee for action by the House, the stage was set for the adoption of two significant amendments.

One of them eliminated the clause forbidding NSF from taking an interest in the scientific affairs of other agencies. The other imbedded in the measure what was to be the much discussed Section 3(a)(6) of the enabling act as later passed. This section obligated the Foundation "to evaluate scientific research programs undertaken by agencies of the Federal Government, and to correlate the Foundation's scientific research programs with those undertaken by individuals and by public and private research groups."

News of these changes produced long faces among the ICSRD scientists. Alan Waterman gave words to their concern. A member of the Interdepartmental Committee by reason of his position as Chief Scientist for the Office of Naval Research, Waterman called "evaluation of the country's research . . . an impossibly large task." It was one, he said, that "a wise and intelligent Foundation would not attempt to carry out."

Noting that the changes in the NSF bill had been requested by the Bureau of the Budget, Waterman concluded that the heads of that agency were "looking to the Foundation to provide them with all the answers on research budgets and also on the performance of all basic research" in the Government. To impose such mammoth duties on the Foundation, he warned, would put scientific "research as a monopoly in the hands of a single Government agency"—a consummation, he believed, that "most scientists" would regard as a "serious mistake."

Certainly the scientists on the Interdepartmental Committee did. But what could they do about it? Waterman was chairman of an ICSRD subcommittee recently set up to keep an eye on NSF legislation. On the evening of January 20, 1950, he and seven members of his group, all scientists, gathered at the Cosmos Club in Washington to ponder tactics.

Should the ICSRD bring its worries to the attention of the House managers of the NSF bill? A brief discussion took care of that question. Passage of the bill was expected momentarily. The consensus of Waterman and his colleagues was that at this late date any attempt to delete the offending amendments would be futile. They decided instead to ask the President to help them.

To this end a letter was drafted, expressing the hope of the ICSRD that after the National Science Foundation began life, Truman would issue a public statement forbidding the Foundation "to move into any area now occupied by existing agencies."[7] In late February, physicist Lawrence R. Hafstad, an ICSRD member, carried the draft letter to John R. Steelman at the White House—and there the maneuver came to a full stop.

Exactly what happened at the White House remains unknown. Hafstad no longer recalls the incident. Neither does Steelman.

"Since the records show that the letter was brought to me," says Steelman, "I must have read it. At that time all sorts of people were coming in to talk about the National Science Foundation bill. Many were agency heads. I remember some of them muttering that they hoped this new agency wouldn't get out of hand—meaning that they hoped it wouldn't take away any of their money. I suppose I told Dr. Hafstad what I was telling everybody then: 'Wait until the bill passes, and hope for the best.' "

One thing is clear. Truman didn't see the ICSRD letter or, if he did, he ignored it. When a few months later the National Science

Foundation got under way, those "difficult coordinating . . . functions" were still troublesomely on hand.

Daniel S. Greenberg, in his *The Politics of Pure Science,*[8] says that before taking on the NSF directorship, Alan Waterman extracted from the Bureau of the Budget an assurance that the Foundation "would not be required to fulfill a role that might bring it into conflict with richer and more influential agencies."

Although the logic of the situation supports the possibility of such an understanding, neither the Bureau records nor those of the Foundation so much as hint at one. To be sure, some of the Bureau staffers regarded the coordination mandate in the NSF act as ambiguous and perhaps unenforceable. Even Bill Carey has said that the original law did not give the Foundation express powers of coordination. He contends, however, that the evaluating functions assigned to it implied such powers; and asserts, emphatically, that "there were no hostages given to Waterman at all to my knowledge."

On learning that ICSRD was seeking a presidential commitment that would free the Foundation from any coordinating responsibilities, the Bureau went into action at once.

"MEET THE ISSUE RAISED BY THE ICSRD CONCEPT. . . .," Carey wrote telegraphically in a planning checklist, drafted as the enabling bill neared passage. "IF HOUSE BILL SURVIVES, NSF WILL BE A 'BUDGET BUREAU' FOR THE FEDERAL RESEARCH AGENCIES IN A SENSE. WE SHOULD GET BEHIND A *STRONG* FOUNDATION, PERHAPS SPELL OUT OUR CONCEPT IN A MEMORANDUM AND GET PRESIDENT TO BLESS IT EARLY IN THE GAME." Eleven days before final Congressional action on the bill, he was writing Staats that the ordinarily somnolent Interdepartmental Committee had "shown an initiative previously unmatched in advising Dr. Steelman to limit severely the scope of the Foundation with reference to research activities of the departments and agencies. Your draft letter should prevent any unfortunate events in this respect."

It is unclear just what draft letter was then in process at the Bureau. In spirit, no doubt, it was similar to the one that Carey took to Alan Waterman on May 15, 1952, and that the NSF Director so suavely shunted aside.

Having failed to lure NSF into a coordinating posture by letters from the Budget Bureau, Carey in the summer of 1953 set out to achieve his ends by means of a more potent weapon—a presidential decree. Interesting are his own recollections of the opening moves in the sequence of events leading to the issuance of this edict—Executive Order 10521—not quite a year later.

"The beginnings of that order, as I remember them," he says, "were that Joseph Dodge was the Director of the Budget and the Eisenhower Administration had come into office [on January 20, 1953] with some of its principal officials, including certain members of the Cabinet, taking a very dim view of scientific research. And the relations between . . . the Administration and the science community were very, very low. And I went to Mr. Dodge one day and told him I thought it was very important for the Administration to demonstrate its support for science, to say something positive since we had been hearing nothing but negative things, and that I would . . . draft for the President's consideration an Executive Order which would have two benefits."

One of the benefits was that the order could emphasize what Carey considered two highly important conditions, namely that the National Science Foundation had the authority to appraise the research programs of other agencies and that President Eisenhower wanted the other agencies to cooperate with the Foundation in such endeavors. The other benefit envisioned by Carey was that the order would "send out a message, both to the science community and to Government agencies, about the real views of the President and perhaps in this way create an environment in which constructive discussions could go on" between the Government and the science community.

Dodge pronounced Carey's proposal a "capital idea" and by July 6 Carey had composed not one but two possible statements for Alan Waterman's perusal. Receiving the draft orders by messenger, Waterman scanned them and then hastened to the Bureau offices to offer some suggestions for revision.

He was not enthusiastic about either order. Not for the first time he pointed out to Carey that the Bureau was simply asking too much of his "small, semi-invisible" Foundation. "Bill," he said, "when we get our budgets up to a high level which other agencies will respect, then we'll be grown up and perhaps we can do some of these things." Carey conceded that to date the Foundation had not enjoyed "a distinguished appropriations history." Still he was determined that it start doing "some of these things" at once.

Reluctantly, Waterman went along. Carey also went along—to the extent of slightly refurbishing his proposed orders to reflect some of the NSF Director's suggested changes. Out of this process emerged two draft documents, conveniently referred to as "the National Science Foundation order" and "the Coordination order."

The NSF order instructed the Foundation, among other things, to study the Nation's scientific efforts and "foreseeable needs," to recommend "appropriate" research policies to the President, and to evaluate the scientific research programs of the United States Government. In addition, the order designated the Foundation as "the primary agency for the support of basic scientific research" with the understanding that the other agencies were to continue funding whatever research of this type their missions required.

The major commands of the Coordination order were directed not at the National Science Foundation but at the other Federal agencies concerned with research and development. Of these injunctions, no less than three would attract vigorous opposition and undergo serious modification. One of them enjoined the agency heads "to ensure . . . that only those research and development projects *which meet the criterion of necessity* [italics added] shall be initiated." Another encouraged each of the interested agencies to share with other agencies any research equipment or facilities that they controlled but were not employing in full. The third of these controversial provisions required the agency heads, "in accordance with policies to be issued by the National Science Foundation," to relieve universities and colleges of any federally sponsored "non-basic research" that could be carried on just as well by industry or by the Government in its own laboratories.

By July 9, both the NSF and the Coordination documents were ready, and from this point on the Bureau followed the procedure common to the maturation of all executive orders.

As a start, copies were sent to a handful of officials whose place in the Federal hierarchy or in the science and business communities was such that serious objections from them could render Carey's scheme difficult, if not impossible, to carry through. One member of the small group receiving these early drafts was Arthur S. Flemming, Director of the Office of Defense Mobilization. Among the others were Hugh L. Dryden, Director of the National Advisory Committee for Aeronautics and chairman of ICSRD; Gabriel Hauge, administrative assistant to Eisenhower for economic affairs; and General Robert Cutler, special assistant to the President for national security affairs.

Flemming, Dryden, and Hauge had a few criticisms, none substantive. More troublesome were the comments from General Cutler, complaints that the Budget Bureau took very seriously because of the vertical position of their author. A Boston bank executive and lawyer, Cutler enjoyed the confidence both of the influential academic fraternity in his home area and of many of the American corporation executives known to be sympathetic to Government endowment of scientific research.

All of Cutler's barbs were aimed at the National Science Foundation order. What did the Bureau of the Budget have in mind in asking NSF to study "the Nation's present and foreseeable needs in

Science?" To the General, that requirement seemed so "broad" that he could not for the life of him see how it could produce anything but "a collection of duck feathers." He had no objections to NSF evaluating the programs of other agencies. But what precisely did the order intend the Foundation to evaluate—basic research only, or was it to look into applied research and development as well? The General demanded clarification on these points.

He got it—fast. In a letter written by Carey and signed by Dodge, the Bureau undertook to quiet the Boston banker's perturbations.

"We believe," the Bureau letter said, that somewhere in the Government there must be "a responsible group of Federal scientific personnel . . . which can comprehensively take the measure of the whole range of scientific effort and see where it is leading us in terms of arms, money, and trained manpower." Pointing out that no such apparatus existed, the letter stated that the proposed orders would indeed give the National Science Foundation "certain responsibilities in the nonbasic fields of research." There was no intention of asking the Foundation to investigate "specific projects" in those fields. Still, it "should be in a position to know the extent and aims of both applied and developmental" work so as to decide if "we are doing too little, or too much basic research; and to predict the future demands of all science for money and manpower."

On the route slip that carried this letter to Dodge for his signature, Carey wrote, "General Cutler is having a hard time understanding our National Science Foundation order. This reply, while fairly long, tries to explain matters."

Apparently it was successful. The General's roar was heard no more. And on August 12, having undergone another slight revision, Carey's draft orders began their most dangerous voyage. Off they went to be cleared by the 23 Federal departments and agencies then involved in some kind of scientific labor.

In his *A Thousand Days*, Arthur M. Schlesinger, Jr., calls the Federal Government's clearance process one of the sharper agonies of bureaucratic procedure. In truth, Carey's draft orders encountered some stormy weather. Granted that 14 of the agencies consulted were not doing the sort of scientific research that the orders would be likely to hamper and so were happy to "interpose no objection" to them. The others had strenuous objections.

Most of the more critical commentators wanted to modify the directive that agencies confine themselves to "those research and development projects *which meet the criterion of necessity.*" Leonard Carmichael, Secretary of the Smithsonian Institution, said that this stricture would fix "boundaries on the scope of research . . . too narrow to permit . . . some agencies" to do effective research of any sort. "It has been proved time and time again, in industry, Government, and the universities," Dr. Carmichael lectured, "that research pays dividends and any attempt to limit it too closely would waste money, rather than save it."

Carmichael echoed the views of several other agencies by advocating the elimination from the orders of the requirement that an agency share its research equipment and facilities with other agencies. Experience had convinced him that such interchanges tended to be frustratingly difficult and expensive. In his concluding remarks, the seventh Secretary of the Smithsonian, a well-known psychologist and biologist, probably gave voice to a conviction common to the American science community as a whole.

"Federal funds (unfortunately it may be)," he wrote, "are essential today in the support of science, but every effort must be made to avoid the deadening effect of too much . . . 'bureaucratic control' of the detailed use of these funds."

Carmichael directed his warmest fire at those clauses of the orders compelling NSF to see to it that the other agencies relieved universities and colleges of federally funded nonbasic research and that they make certain that scientific grants to such institutions did not "imbalance" their academic programs as a whole. The Smithsonian secretary predicted that the retention of such decreta in the orders "would be made the basis of criticism on the part of higher education both in and out of Congress." He considered this "cumbersome machinery . . . in fact unnecessary since no educational institution is required to accept grants or contracts, and they can themselves protect their educational responsibilities adequately."

Addressing himself to the same problem, Secretary of Agriculture Ezra Taft Benson went a step further. He said that the slightest suggestion in the proposed orders that some research funds be withdrawn from educational institutions would strike terror into the hearts of their overseers. As administrator of the experimental-station funds distributed to land-grant colleges, Benson had discovered that institutions of higher learning would like to have "not fewer but more research grants and contracts."

Secretary of Labor Martin P. Durkin let it be known that some of the statements in the proposed orders treaded on his toes. "The National Science Foundation Order," he asserted, "may give the Foundation a greater degree of centralized . . . power than does the statute which created it. The order, for example, directs the Foundation to make comprehensive studies of the Nation's manpower resources and demands on available scientific manpower, a function vested in the Department of Labor. . . ." In much the same vein, Chairman Johnson of the Interstate Commerce Commission noted that his agency's scientific pursuits were "determined by law." It followed that "unless Congress should provide otherwise," the ICC could not be "amenable to orders of the National Science Foundation."

In a five-page, single-spaced letter, frothing with annoyance, the Chairman of the Atomic Energy Commission, Lewis L. Strauss, asserted that the "proposed executive orders . . . would make far reaching changes in the administration of the Government's research effort which would not be in the public interest." According to Strauss the difference between research in atomic energy and that handled by other agencies was enormous. "Centralized management of governmental science" by NSF would, therefore, be "undesirable."

It did not escape the attention of Admiral Strauss that considerable nonbasic research went on in the university-operated laboratories owned by the Atomic Energy Commission. He assumed that the provision in the orders designed to reduce this type of research at educational institutions did not apply to AEC properties. "If this assumption is not correct," he warned, "we would have an additional major objection. . . ."

Writing as Secretary of the newly created Department of Health, Education, and Welfare, Russell R. Larnum took issue with the restrictions that the orders placed on duplication of scientific effort. He pointed out that in basic science some duplication was "unavoidable and acceptable."

As for the National Science Foundation, its rather lukewarm commentary included a statement from Waterman that occasioned no surprise in the offices of the Budget Bureau. As the proposed orders merely confirmed certain duties already demanded of NSF by its enabling act, the Director saw no point in bothering with them at all. Why not avoid "many . . . difficulties of interpretation," he asked, by substituting a simple statement "covering the same ground but permitting less formal . . . discussion of the policies involved?"

Fully aware that Bill Carey would not fall for this effort to rationalize the orders into oblivion, Waterman then proceeded resignedly to endorse most of the changes that the other agencies had advanced. His most emphatic statement was that a literal interpretation of the move to withdraw Federal sponsorship from nonbasic research in universities and colleges could work incalculable harm. He reminded the budget people that at the request of the Government "many of these institutions" had long since undertaken a fairly large amount of this kind of work. "As a result," he observed, "some have made extensive commitments and in certain instances costly additions to their plants. . . . The termination of work already under contract . . . may well be both financially embarrassing to the institution and disorganizing to scientific effort."

All in all, by the time the last critique reached the Budget Bureau in late September, Carey and his assistants had their hands full. Which of the suggested alterations should be accepted, which ignored? HEW Secretary Larnum had written that the two orders were redundant and should be combined into one. A good idea, Carey concluded, with the result that an intensive revision during the first half of November eventuated in a single draft document entitled "Concerning the Administration of Scientific Research by Executive Departments and Agencies."

It reflected some, although by no means all, of the proposed changes. Gone was the clause advising agencies to limit themselves to "necessary" research. Gone was any threat to the nonbasic research under way in universities and colleges. And the section dealing with the impact of federally supported science on academia stressed the necessity of "safeguarding the strength and independence of the Nation's institutions of learning."

Instead of being singled out as the Government's major sponsor of basic research, the National Science Foundation was made "increasingly responsible for providing the support by the Federal Government for *general purpose* basic research through contracts and grants [italics added]."

The recommendation that agencies share their research equipment and facilities remained. Its sting, however, was drawn. A "Federal agency," the order said, "shall procure new major equipment or facilities" only after making sure that its needs could not be readily met by already existing ones.

On its second circulation of the agencies, completed soon after the turn of the year, the combined order fared well. Even AEC was mollified, following a personal conference between Waterman and AEC Commissioner Henry D. Smyth, at which the Director assured the Commissioner that the National Science Foundation had no intention of going into the atomic energy business.

On St. Patrick's day, a buzz at the White House signaled the occurrence of a Presidential press conference. Mr. Eisenhower had a number of front-page revelations to share with his note-taking audience. He had seemingly completed this opening remarks when an afterthought overtook him.

"There is one other little item," he said, "an Executive Order that will be published, I guess, this afternoon, having to do with this research and development. The only reason I mention it is because of the tremendous impression . . . it makes on me when I look at the sums that Government spent for research and development only . . . 12, 13 years ago and what we are spending now. When you have an item of more than 2 billion in your budget, you have something that takes, of course, not only the finest scientific brains you can find in the United States to supervise and coordinate it, but is really big business of a very large order going from, I believe, 1940 [when the research budget] . . . in all departments ran to 100 million. I think that Mr. Hagerty will have a statement to put out along about four o'clock."[9]

As a matter of fact, Press Secretary James C. Hagerty did not get around to it until two days later: On March 19, 1954, Executive Order 10521 finally materialized.

On the same day that the Executive Order became official, Waterman sent a copy of it to his physicist friend, Lee Alvin DuBridge, President of California Institute of Technology in Pasadena and member of the National Science Board. The NSF head expressed the hope that now that "the tumult" was over the order would be "properly interpreted" and prove "on the whole" to be beneficial. "At any rate," he promised DuBridge, "we at the Foundation will try to uphold the proper interpretation."[10]

But what was the "proper interpretation?" On March 22, that question came up at a meeting of the senior staff of the Foundation. And at least one member, William Jay Hoff, returned to his office slightly troubled by the positions taken by most of his colleagues.

A trim and steady-eyed New Yorker with a refreshingly courtly yet easy manner, Hoff had only recently joined the Foundation as General Counsel. On the 25th he sent to Waterman a personal memo in which he took the liberty of departing from his legal capacity "to offer a few comments on the policy role of the Foundation."

Hoff informed the Director that he recognized "the importance of the Foundation's operational responsibilities as the only agency in the Government dedicated solely to the support of basic research." But should NSF concentrate on these pursuits to the neglect of others? Thinking back on the "discussion at Senior Staff on Monday," Hoff found himself "somewhat perturbed at the pervasiveness with which our concern with operations colored our thinking on the Foundation's policy responsibilities." It seemed to him that "the extent of the contribution the NSF can make . . . to national progress in science by vigorously undertaking" its policy-setting tasks could not "be overestimated."

Dominating the counselor's thoughtful memo was a plea that the NSF Director give close attention to the emphasis that Executive Order 10521 placed on the Foundation's duty to advise "other agencies . . . on matters relating to needs in research and administration of research . . . ." In this provision of the Executive Order Hoff discerned "an invitation to the Foundation to assume a major role" in the governmental setup. "Such an opportunity," he told Waterman, "may not present itself again."[11]

Reading back from subsequent events, it is safe to say that Hoff's interpretation of Executive Order 10521 was not the "proper" one that Alan Waterman had in mind. But while Waterman continued to run his agency pretty much as he always had, the movers and shakers at the Bureau of the Budget were not idle. Bill Carey's tireless hand is visible in their efforts, initiated during the closing months of 1954, to test the ability of the National Science Foundation—or, more precisely, its willingness—to monitor the scientific activities of other agencies.

This development began in the late fall when Waterman and National Science Board Chairman Barnard received what they called an "informal" or "confidential" request from Rowland R. Hughes, successor to Joseph Dodge as Director of the Budget.

Hughes' request had to do with the extensive medical research programs administered by the Department of Health, Education, and Welfare and conducted for the most part by the seven large laboratories in Bethesda, Md., known collectively as the National Institutes of Health or NIH. Of recent years HEW's medical research budget had grown impressively. Simultaneously the task of properly distributing ever larger sums among the numerous research centers of her sprawling Department had become increasingly difficult for HEW Secretary, Mrs. Oveta Culp Hobby.

Budget Director Hughes' proposition was that NSF give Mrs. Hobby a helping hand. The Foundation could do this, he said, by scrutinizing her Department's research programs and especially those under the aegis of the National Institutes of Health.[12]

Obviously there was a potential payoff in this proposition for the Bureau of the Budget. Research appropriations for HEW had enlarged to the point where its fiscal year 1954 allotment for such purposes stood close to $66 million. Conceivably, an investigation of its medical research by a group of NSF-picked experts could supply the Bureau with the kind of information that might persuade even a health-minded Congress to be a little less lavish in the future. The fact remains, as Bill Carey has said, that Budget Director Hughes' proposal to NSF in late 1954 was primarily "a probe to find out whether the Science Foundation, given an explicit request from the Executive Office of the President, could indeed pull up its socks and be useful to the Executive Office in science policy and evaluation."

Having digested Hughes' request, the Foundation proceeded to do all the proper things with the eagerness of an unarmed man stepping into a den of lions. At its November 5 meeting the National Science Board set up a special six-man committee to consider the Budget Director's proposal. Chairman Barnard chose Dr. Bronk to head this group and assigned Dr. Waterman to it in an ex officio capacity. By

November 22 the Bronk committee had prepared a draft letter for transmittal to Hughes and Secretary Hobby.

The letter said that the National Science Foundation had been alerted "to the imbalance of programs of research in the National Institutes of Health resulting from changes in requested appropriations due to overemphasis on categorized areas of research."[13] This stately language referred to the fact that each of the seven Institutes of Health confined its investigations to one or two particular diseases or at most to a restricted aspect of medical research. Naturally, in making its yearly requests for funds, each Institute tended to overemphasize the importance of its own work—a situation that put Secretary Hobby in the position of King Solomon dividing up the baby. Her annual headache, in a nutshell, was to decide how much of the Department's research money each of the Institutes actually deserved.

NSF's draft letter outlined a solution to the problem. It urged Mrs. Hobby to create an advisory commission, with overall reviewing powers, to assist her in arriving at proper budgetary determinations. In short, having been invited to pronounce judgment on HEW's medical research, the National Science Foundation was now proposing that HEW be its own judge and jury.

It would appear that the NSF draft letter never went officially to Hughes and Mrs. Hobby. Bronk and Waterman, however, presented its major recommendation to HEW Undersecretary Nelson A. Rockefeller. His comment was that an advisory commission appointed by the HEW Secretary would not be "entirely adequate in objectivity," a scruple that Bronk and Waterman proposed to circumvent by letting either NSF or the National Academy of Sciences recommend the commission members.

There followed other talks between NSF leaders and the Bureau, culminated on December 9 by a long conference at Foundation headquarters. Waterman, Bronk and Barnard represented their agency at this confab. Hughes, Carey and Assistant Budget Director Donald R. Belcher were on hand for the Bureau.

The NSF leaders reiterated their conviction that the Department of Health, Education, and Welfare should itself inspect its research programs. Waterman went so far as to quote Scripture—which is to say that he found justification for his position in the wordage of Executive Order 10521. He pointed out that the HEW's National Institutes of Health were engaged to some extent in phases of applied research such as clinical medicine and hospital care. A review of their programs, therefore, would go beyond the basic-research premises staked out for NSF. It would thus force the Foundation into places where, as Waterman read the Executive Order, NSF would be a trespasser. Waterman also cited the apprehensions harbored by some of his Board members. They were fearful that an assessment of HEW's research programs would engender hostilities likely to impair the whole program of support for the medical sciences.

Neither these arguments nor any others moved Hughes and his assistants. The budget Director said that the proposed review by NSF was desperately needed. He trusted that the Foundation would "not draw back for fear of consequences." Carey summed up his agency's position by saying that the President, in seeking a solution to the administrative tangle at HEW, naturally expected action by the Foundation as it had the authority to examine the research efforts of other agencies. Hughes then closed the session with a suggestion. He urged Waterman to draft a letter asking NSF to make the review—a letter that Mrs. Hobby would sign and send to the Foundation. Reluctantly Waterman agreed to this procedure.

Let it be confessed at once that the rest of this tale is anticlimax. In August 1955 the National Science Board created an eight-man committee headed by Dr. C.N.H. Long of the Yale University School of Medicine. Assisted by a secretariat under Dr. Joseph Pisani of the State University College of Medicine at New York City, the committee devoted 2 days a week for 3 months to examining both NIH and the other medical research programs of HEW. On December 5, 1966, its report, a 64-page document, was accepted by the National Science Board.[14]

John T. Wilson—Waterman's Program Director of Psychobiology in 1955, later Haworth's Deputy, and now President of the University of Chicago—recalls the "Long Report" as being a "pretty hot item." Bill Carey disagrees. Too "flattering" an evaluation of it, he contends.

According to Carey, the Long Report "was not that helpful. It was a situation where hardly any objective experts could be assembled to conduct that evaluation for the reason that they were dipping their spoons into the medical research budget, and the future of their institutions and their professional goals depended on the good will of NIH. And from where we sat it was a pallid evaluation which danced around the critical issues and made brave noises on the ones that were not critical. And it told us, if we needed any more telling, that it was going to be a long time before NSF took seriously its policy function."

By the time the Long Report was ready, Mrs. Hobby was no longer at HEW. The report went to her successor, Marion B. Folsom, who dropped it into his files where presumably it still reposes, unacted upon and forgotten.

Carey was correct in his prediction that it would "be a long time" before the National Science Foundation took its policy function seriously. In truth, however, it was primarily world events beyond the reach of NSF that in 1959 brought forth another Executive Order that in effect relieved the Foundation of its coordinating and evaluating burdens. What this presidential ukase (Executive Order 10807) did was install at the presidential level a new interagency organization called the Federal Council for Science and Technology.[15] As Budget Bureau officer Clifford Berg noted, by giving Director Waterman only one of the nine voting seats on the Council, the order of 1959 "decimated" the Foundation's "initiative and coordinative responsibilities."

Three years later a sweeping reorganization plan went further. This edict established in the Executive Office the Office of Science and Technology and categorically transferred to that body all of NSF's coordinating chores and most of its policy functions.

Did Waterman act wisely in refusing to grasp the nettle, coordination? The question even at this late date is not academic if only because at the moment (1975), thanks to the recent dismantling of most of the science-advisory apparatus at the White House, many of the burdens Waterman avoided now rest on the shoulders of his current successor at the helm of NSF, Dr. H. Guyford Stever. Unsurprisingly, some informed observers say Waterman was justified in what he did, and others say he was not.

Speaking as a charter member of the National Science Board and for many years its able Chairman, Dr. Detlev Bronk says flatly that for "that little Foundation to have tried to coordinate the entire Federal research program in the 1950's would have been arrogant, presumptuous, and disastrous."[16]

Don K. Price of Harvard resorts to "an analogy" to show that it would also have been difficult, perhaps even impossible. "A junior colleague of mine, with an associate," Dean Price recalls, "wrote a book [about the science-Government relationship] that was published a few years ago. . . . They were very good scholars but they were very young, and in the first draft of that book, which they showed me, they said that one of the functions during World War II of the Office of Scientific Research and Development was to coordinate the research programs of the War and Navy Departments. And I said, 'where in the hell did you get this idea?' And they showed me that there it was in the Executive Order setting up the Office of Scientific Research and Development. And I said, 'look here, I don't care what that Executive Order says. This just shows you how little official laws mean in many cases.' It was inconceivable that a small civilian organization could run or direct those two vast military bureaucracies."

By the same token, according to Price, the National Science Foundation, as set up in 1950, was "just the smallest and youngest and weakest of the Government's scientific research programs." As such it could do only what it did do: It could marshal data and pass them on, with a little "dispassionate advice, to a place which really has got some muscle and leverage, and that's the Budget Bureau."[17]

Those who take the other side of the argument say that had Waterman boldly tackled his evaluation and policy functions, the National Science Foundation would not have remained small for very long. They contend further that with the backing of the Budget Bureau the Foundation could have carried out its coordination duties successfully.

Former NASA Administrator, James Webb, recalls that one of Truman's reasons for insisting on his right to appoint the NSF Director, rather than let the National Science Board do so, was that he expected the Foundation to play a role in the correlation of the Government's science program as a whole. It follows, in Webb's view, that in failing to accept a role in performing certain of the coordinating functions, Waterman "was operating against the history of what the President had objected to when he vetoed the first NSF bill—that is, having a Director appointed by a Board and empowered by law to perform what he regarded as a presidential function. Webb adds, however, that "Dr. Waterman was a very wise person. He recognized that there were certain limits to what an organization could do. . . . There had been a definite statement by the President in his veto message that he thought this [coordination] was part of his own responsibility, and even though I think Truman would have been glad for Waterman to do it, Waterman saw many reefs and shoals and rocks against which the effort could be wrecked. I think he had real concern as to whether he could be effective in that role."

Could any NSF Director be effective in it? Confronted by this question, Webb says, "He can do it to the extent the President wants him to do it and supports the man. The plain fact of life in Washington in these areas, either at the White House level or the Executive Office staff level or the agency level, is that you can do a very large and effective job of coordination if the President is behind you."[18]

Helpful in understanding Alan Waterman's handling of the coordination issue is a remark known to have been made during the early days of the Foundation by Lloyd V. Berkner. An internationally known physicist and science administrator, Berkner had been among the men whom President Truman had considered as a possible NSF Director. Uttered a few years after the Foundation got under way, the famous physicist's comment ran somewhat as follows: "It's a good thing they picked Alan. He's been slow and cautious, and sometimes people have been irritated by the way he has handled things. But he has been a very steady and constructive builder. Had I been the

Director I would have moved too fast, and the Foundation would probably be torn apart by now."[19]

The argument remains an open-ended one, about which only one firm statement can be made. Waterman's refusal to embrace the touchy problems of coordination had the effect of determining the tone and direction of the National Science Foundation by freeing the agency, at an early date, to concentrate on its primary operating function, the promotion of fundamental research and training in the sciences.

Chapter 6. To Coordinate or Not to Coordinate

[1] Except as otherwise indicated, the information in this chapter is based on BOB 39.32 and 53.2e, NA.

[2] Personal interview. Other quotes from Carey in this chapter are from the same source.

[3] Phone interview.

[4] Executive Order 9912, Dec. 24, 1947, *1947 Supplement to the Code of Federal Regulations, Titles 1-7.*

[5] *Cong. Rec.*, 81st Cong., 2d Sess., pp. 2412, 2415, 2426, 2437-9, 2442, 2444.

[6] Ltr., Staats to author, Feb. 5, 1973.

[7] ATW Personal Diary: Meeting of subcommittees—6:30 p.m., Cosmos Club, Office of Director Subject Files, NSF 66-93, Container 8, Washington National Records Center.

[8] New York, 1967, p. 145.

[9] Excerpt from President's Statement at Press Conference, Mar. 17, 1954, Office of Director Subject Files, NSF 67-15, container 2, Record Group 307, Washington National Records Center. See also Executive Order 10521 in United States Code, 1954, pp. 1824ff.

[10] Mar. 19, 1954, Chron File.

[11] May 25, 1954, Office of General Counsel files.

[12] Memo, ATW to Rowland-Hughes, No. 10, 1954, Chron File. See also NSB Min. 30:3.

[13] Memo, ATW to Bronk and others, Nov. 23, 1954, Chron File.

[14] NSB Min.: 37:Appendix I.

[15] *U.S. Code 1959*, pp. 104ff.

[16] Personal interview.

[17] Personal interview.

[18] Personal interview.

[19] Ltr., Dael Wolfle to author, Dec. 17, 1973.

Chapter 7

The First Two Directors

Shortly before Congress passed the final NSF bill and Truman signed it, physiologist Ralph W. Gerard of the University of Chicago warned that the success of the new agency would depend on the ability of its leaders to educate "public and Congress to the meaning and importance of basic research."[1]

None of the men responsible for the early development of the Foundation seems to have been more responsive to this challenge than its first Director. Eventually the Foundation acquired a staff of experts to assist in handling its relations with Congress, but for many years the Directors did most of this work on their own. It would appear that Waterman performed his liaison chores with a minimum of fanfare, for to this day a veteran NSF staffer proudly insists that "the first Director was so nonpolitical that he wouldn't even accept phone calls from Congressmen."

In point of fact, Waterman accepted such calls religiously, initiated many of them himself, and took pains to keep on good terms with Albert Thomas of Texas who, as chairman of the House Appropriations Committee's Subcommittee on Independent Offices—the body charged with scrutinizing the Foundation's budgetary requests—was for many years the key man on the Hill as far as NSF was concerned.

As the *New York Times* once observed, Waterman was not an assertive man and was therefore not particularly well endowed for the public relations tasks implicit in his effort to alert the American public to the necessity of enlarging the Nation's stockpile of scientific knowledge. Whatever natural talents he lacked in this department, however, he made up for by hard work and a crusading spirit.

He had no hesitation, for example, about writing editors of scientific periodicals, suggesting that they run stories on the Foundation, or better yet, devote whole issues to its aims and deeds. Whenever his attention alighted on a newspaper article or a speech favorable to the agency's objectives, he lost no time in sending a letter of appreciation to its author. He was equally quick to pounce on published statements that struck him as erroneous or unfair.

He himself contributed occasionally to scientific publications, and a letter to the editors of one of them found him good-natured about a common hazard of the writing profession. "An author," he noted:

> must be prepared to accept philosophically an occasional typographical error, even when it occurs in so impeccably edited a journal as the AMERICAN SCIENTIST. In the case of my paper, "Science in the Sixties," which appeared in the March issue, I must, however, respectfully beg you to publish a correction in the Greek words . . . at the end. Not only is the honor of Princeton at stake (where I qualified in Greek in order to receive my A.B. degree) but also the honor of the Society of Sigma Xi. As it stands . . . the Greek now reads, "Companions in Libations," instead of "Companions in Zealous Research." I should not like to stand as the sole sponsor of so drastic a change in [Sigma Xi's] . . . purposes as expressed in its motto.[2]

The Director's office door stood open to reporters. When a news magazine editor asked why Congress was so reluctant about giving the agency an adequate budget, Waterman answered that NSF's major product was "hard to sell." Basic research, he explained, is "like a girl with virtue. It's a nice thing to have but the man who's going to support her has trouble getting his mind off what's going to come out of the cooking pot." Happily for the Director's image, this racy definition of pure science appears to have found its way into only one printed copy of the magazine. At that point the editorial blue pencil fell, and the copies that went out to subscribers and newsstand buyers carried a more chastely worded version of the Director's thought.[3]

An incident of 1951 brings out Waterman's abiding fear that his agency was then such a fragile affair that a puff of criticism might blow it away. The spring of that year saw the completion of Lyman Chalkley's never-published manuscript, covering the legislative background of the Foundation. Chalkley had prepared the account at the urging of the Columbia University Seminar on Organization and Management. Entitled "Prologue to the U.S. National Science Foundation," his recital differed rather sharply from what was already becoming the official version of NSF history, according to which Vannevar Bush simply lifted his hand one day and said "Let there be a National Science Foundation," and there was one. Chalkley dwelt at some length on the often acrimonious arguments and clashes of interest and principle that had marked the 5-year struggle in Congress to bring NSF to life. He listed both Senator Kilgore and his assistant, Herbert Schimmel, as "fathers" of the Foundation along with Bush.

Having reviewed the manuscript, Waterman sent a memo to the National Science Board, expressing the hope that Chalkley's pamphlet would not see print. In a letter addressed to its author (apparently never sent), he stated his reasons for this stand.

"You are, of course, aware," he wrote, "of the drastic action of the House Appropriations Committee with respect to the National Science Foundation's appropriation for the first year [its attempt to deprive the agency of any budget at all for fiscal year 1952.] This action has in effect reopened the whole question of whether there should or should not be a National Science Foundation, and has placed us somewhat in the position of having to justify its existence all over again. For a book to appear at this particular time, in which all the issues which were most bitterly argued are brought forth again in such a manner as to provoke fresh controversy, would be most unfortunate."[4]

However exaggerated Waterman's anxieties may seem in retrosprect, they were an understandable outgrowth of his concern at the sluggish financial progress of the agency during its opening years. Even after the $15 million ceiling on appropriations vanished in 1953, a Congress caught up in the economy drive generated by the Korean War continued to keep the Foundation on an austerity regimen. (Vannevar Bush, incidentally, once remarked that he urged Congress to do this on the grounds that it would be good for the Foundation "to have to earn its keep.")[5] For fiscal year 1954, NSF asked for $15 million and got $8 million. For 1955 its request was $14 million, its appropriation $12.5 million. Not until 1956, when a request for $20 million brought $16 million, did the budget exceed the abandoned ceiling.

The first strikingly substantial rise came in fiscal year 1959, following the launching by the Soviet Union of Sputnik I—a technological triumph in that it marked mankind's first successful effort to put an artificial Earth satellite into orbit. As many readers will vividly recall, this affront to long-held assumptions of American scientific and technological superiority had its effects. NSF's last pre-Sputnik appropriation was $40 million. Its first post-Sputnik budget was $130 million.*

For the next 9 years appropriations climbed steadily. In 1963, Waterman's last year as Director, the appropriations stood at $322.5 million. In 1968 it was $495 million. Although the budget dropped substantially in 1969, another large rise lay in the offing. For purposes of tracing the growth of the Foundation, however, the first 18 years can be treated as a unit for the reason that the priorities crystallized during Waterman's 12 years in office continued to prevail throughout most of Haworth's 6.

Both of the first two Directors operated on the principle that what was good for pure science was good for the country. Both thought of science as moving as it were on its own momentum, admittedly influenced by outside pressures, such as economic or military needs, but in the long pull never directed by them. Neither took kindly to suggestions from Congress that the Foundation devote more of its funds to undertakings aimed at solving specific national problems. Describing fundamental science as the "defense in depth for our whole technology," Waterman warned that were basic research "too strongly warped in the direction of immediate technological goals, the overall technical strength of the country" would diminish because it could then "only undertake foreseeable objectives and miss innovations.[6] Haworth occupied the same ground. He characterized undirected basic research as an "investment in the future." Although "some of the research supported by the Foundation is problem oriented," he said in 1967, "it nevertheless must hold the promise of yielding valid scientific generalizations rather than being limited to finding solutions for problems peculiar to a particular time, place, or interest."[7]

---

* Exclusive, in both cases, of small supplemental appropriations.

Although a profound personal commitment to the importance of fundamental research undergirded these attitudes, both men were also acutely conscious that the original NSF enabling act explicitly limited the support activities of the Foundation to that type of science. Waterman made a point of this fact when in 1960 the President's Science Advisory Committee, in a draft of a report on government research and development, urged NSF to engage "increasingly . . . in studies to provide recommendations covering national needs in areas of research, both basic and applied. . . ." The NSF chief objected to this statement on the grounds that it implied "that the Foundation should stretch its legal authority." Such a suggestion, he told the President's Science Adviser, George B. Kistiakowsky, "would be regarded as out of order in many quarters and does not, to me, seem necessary. If this is to be stated it should be done in the form of a recommendation to change the statutory authority of the Foundation."[8]

Haworth took the same position. On assuming the directorship he was troubled to find that in actuality some of the work being funded by the Foundation might be considered to be applied research and hence outside its legal authority. Years later he would recall discussing the matter with Congressman Emilio Q. Daddario, a key member of the House Committee on Science and Astronautics. He remembers suggesting to Daddario that Congress "make the Foundation honest" by amending its charter so as to permit the agency to support some applied research. A few years later, as we will have occasion to note in a subsequent chapter, this suggestion would bear fruit.[9]

Both Waterman and Haworth looked upon the country's academic community, in Haworth's words, as "the chief component of the public" that the Foundation was obliged to serve.[10] At an early date, to be sure, Waterman and his staff gave thought to providing some assistance to scientists working on basic problems in industrial laboratories. This possibility fell to the ground in 1955 when a highly respected atmospheric scientist connected with a California instrument firm submitted a "tentative" request for funds to undertake a fundamental investigation in cloud physics. Waterman himself had encouraged this action as a means of testing the willingness of the National Science Board to consider such proposals.[11] Although the record is silent on how the Board disposed of the matter, it is plain that the members concluded that it would be inappropriate for NSF to underwrite the pursuit of pure science by a profitmaking organization. Such at any rate has been Foundation practice. Proposals from industry for the support of projects in applied or problem-oriented research are entertained, but where uncommitted basic research is concerned, NSF normally accepts applications only from universities and other nonprofit institutions.*

In relaxed and reminiscent moments, Dr. Haworth is fond of telling a couple of stories about his ancestors. The first American among them was a Quaker who came to the New World with William Penn in the late 17th century. His point of origin was a small town in Yorkshire, England, called Haworth, now famous as the village where the Bronte sisters grew up and began their literary careers. At first the American Haworths pronounced the family name, not as in "Hayworth," as they do now, but as in "Hah-worth," as the village is known. Then, in the late 18th century, having accompanied Daniel Boone in his first and unsuccessful attempt to settle what is now Kentucky, some of the Pennsylvania Haworths found themselves in a frontier community where there was a family called Howarth and one called Howard. So great became the confusion that the Haworths obligingly changed the pronunciation of their name.

Dr. Haworth's other tale concerns another Carolina-based ancestor named MacDonald. "The whites of his region," Haworth relates, "attacked a Cherokee village, and proceeded to kill everybody, men, women, and children. And this man, MacDonald, found two little girl babies, obviously twins, and he protected them and took them home and raised them, and his son married one of them. And she was one of my ancestors."

---

* This matter did come up for discussion at a meeting of the Board on January 23, 1959. The minutes show that it "appeared to be the consensus that the support of basic research in industry should be undertaken only after careful consideration" and only under these conditions: that the research "was of special concern from a national point of view," that "the industrial concern had special facilities or unique competency to perform the work," or that "the basic research is important but is not related to the mission of the industry." (NSB 58:14)

Like his predecessor at NSF, Haworth, when he took over the Foundation on July 1, 1963, could look back on a long association with academia. At the time of his birth in Flint, Mich., on July 11, 1904, his father, Paul Haworth, was completing his graduate work in history at Columbia University. The elder Haworth would devote most of his adult life to the writing of history, first as an employee of a publishing house in Cleveland, Ohio, later as a free-lance author and part-time fruit-tree cultivator, and still later in connection with his chairmanship of the department of history at Butler University in Indianapolis, Ind., a position he had held for some 16 years at the time of his death in 1938.

The younger Haworth, meanwhile, had selected science rather than the humanities as his bailiwick, a decision dictated by his encounter with an inspiring teacher of elementary physics during his sophomore year at Indiana University. By the time he received his doctorate from the University of Wisconsin in 1931, he was already embarked on a long teaching and research career that had begun in an Indianapolis high school.

He was an instructor in physics at the University of Wisconsin when in 1937 a Lalor fellowship enabled him to spend a year doing full-time research in physical chemistry at Massachusetts Institute of Technology; and during World War II he returned to MIT on a leave of absence from the University of Illinois faculty to work as a researcher in the OSRD-funded Radiation Laboratory. He was on his way to becoming one of the country's recognized authorities on the design of high-energy accelerators when in 1947 he left the University of Illinois to serve first as Assistant Director and then as Director of Brookhaven National Laboratory, a large research center on Long Island, N.Y., concerned with all aspects of nuclear science and run under a contract with the Atomic Energy Commission by Associated Universities, Inc.

Sponsored by nine universities—Columbia, Cornell, Harvard, Johns Hopkins, MIT, Pennsylvania, Princeton, Rochester and Yale—AUI dates from 1946 when it was incorporated under the education laws of New York State to establish and operate large-scale research facilities for the use of this country's scientific community. Haworth was serving in two capacities, as director of the laboratory and president of the corporation, when in February 1961 John F. Kennedy named him to the Atomic Energy Commission.

There, according to the *New York Times*,[23] Republican Haworth* quickly developed "into a powerful figure who combined his scientific prestige and political acumen to fashion a sphere of influence that stretched" from the Democratic White House into the AEC staff. "At times," the newspaper added, "there were jealous grumblings from [some of] the other commissioners that his influence was too great and his perspective . . . too narrowly oriented toward science." Presumably there were sighs of relief from these quarters when in 1963 Haworth resigned as commissioner to succeed Waterman as Director of the National Science Foundation.

---

* Of his political leanings, Haworth says that by the time he became an AEC Commissioner "I was registering as a Republican in Long Island only because that was the only way you could influence local politics, it was just so completely Republican. Since then I've never registered for either party and have really been an independent. My father was a Republican, but a Bull-Moose Republican."

He was 59 that year, a rather large man with somewhat rounded shoulders, a tendency to stoop and professorily somber countenance under a bristle of short white hair. His first marriage (in 1927 to Barbara Mottier of Indianapolis, who died in 1961) brought him one daughter and one son. His second (on May 15, 1963) was to Montana-born Irene Benik, who at the time of the marriage was secretary to Jerome Wiesner, science adviser to President Kennedy. In his earlier years he engaged in sports, including tournament tennis and varsity and semi-pro baseball. Science is not Haworth's only love. He is an ardent gardener and an ardent amateur photographer, given to insisting on many angles from different positions with different camera settings. He is also an inveterate reader and buyer of books. His second wife once complained that "we can barely get into the apartment for the books."

One of the revelations of the already cited *New York Times* profile is that on joining the Atomic Energy Commission, Haworth immediately saw to it that blackboards were installed in his office and in all AEC conference rooms. At the National Science Foundation, staff members were quick to notice the new Director's penchant for rushing to the blackboard to illustrate even simple instructions with rapidly scrawled charts and tables. On one occasion he effectively baffled some of his assistants by chalking up a series of mathematical formulas to explain the changes he wanted in a Foundation program. Fortunately one of the aides did understand the equations, shared his knowledge with the others, and couched his report to Haworth on the progress of the requested changes in mathematical formulas.

Haworth's philosophical affinities with Waterman notwithstanding, his administrative methods were distinctly his own. Waterman, as a colleague once remarked, tended to run the Foundation "out of his vest pocket." Even so he ran it loosely, regularly delegating responsibility to his aides. Haworth was a perfectionist, whose close attention to small matters was often a source of exasperation to staff members. A reiterated assertion at headquarters in his day was that "the Director spends too much time on details any stenographer could handle." Once, if persistent scuttlebutt can be trusted, a three-page letter prepared for the signature of the Director so dissatisfied him that he revised it 13 times, eventually enlarging the document to such an extent that in the end he found it necessary to preface it with a separate three-page letter, summarizing its contents.

There is no need to go to others for an appraisal of his administrative style. Visited in the office he now uses at Brookhaven as special assistant to the president of AUI, Haworth turns out to be his own most severe critic. He makes no bones about having been "fussy" about the documents drafted for his signature at NSF. "I found many of them had ambiguities along with typographical errors and faulty English," he says. "So I made it a practice to read every word of every one of them. I don't deny it."

Asked if he now regrets any of the decisions taken during his directorship, he replies ruefully, "Lots of them." If he "had it to do over again," he says, he would make his decision more promptly and aggressively. "I was too slow," he says, "I was not active enough in regard to Congress. I could not bring myself to go and see a Congressman, unless I had something to see him about. I would not take his time just to butter him up, so to speak." Today Haworth feels that it might have been wise to have supplemented his business trips to the Hill by a few exclusively social visits.

He attributes what he calls his "passivity" in part to personal temperament. "Terribly shy by nature." Thus he describes himself. He points out, also, that in "1959, which was 4 years before I came to the Foundation, I had a rectal cancer, and I had a very serious major operation, and ever since then I've had a colostomy. I mention this because I understand that a lot of people face this problem fearfully. It may help them to know that, in spite of it, I managed for a good many years to cope with the Washington rat race."

The same respect for the truth that moves Haworth to mention what he considers to have been some mistakes in his management of the Foundation impells him to describe as "unfounded" an assertion by some of the oldtimers that he "delegated almost nothing." He points out that shortly before his assumption of the directorship he learned that Waterman himself was signing every one of NSF's thousands of grants. Once on the job Haworth turned over this enormously time-consuming task to his newly appointed deputy, John T. Wilson. He would have passed the job even farther down the line had the National Science Board heeded his request to do so.* In addition he changed the heads of the major divisions of the Foundation from staff officers to line officers, thus enlarging their power to make decisions.

---

* Prior to 1959 the Board had to pass on all grants, even on $100 travel grants. Congressional action that year empowered it to delegate the bulk of these tasks. (AR 1959, p. x)

It is worth pointing out that some of Haworth's critics temper their remarks with words of admiration for the man himself. Not untypical is this statement from a longtime member of the NSF staff:

"When Haworth came into the directorship the sun was shining on American science, but during his regime the weather became pretty stormy, as the Sputnik fever passed and the Vietnam War in Asia and the War on Poverty here ate into available funds and lifted costs. When he left, however, the skies were clearing, largely thanks to his efforts. The key to the man's character was integrity. The Foundation benefited greatly from that. Taken as a whole, his directorship was more effective and helpful to NSF than he himself realized."

In sunshine or storm the overall tone and thrust of the Foundation remained substantially the same during Waterman's regime and during most of Haworth's. Not that there were no changes during this period; there were many, but generally speaking they pushed the Foundation more in the direction it was already going.

Not until the closing phase of these formative 18 years did a change in public attitudes, including a growing demand for more scientific attention to national difficulties, presage a change in the Foundation's operating policies. Not until the last year of the period did Congress so amend the NSF enabling act as to require the agency to reconsider its priorities.

How the Foundation has coped with the new directions imposed on it by the legislation of 1968 can be left to a later point in this account. How it grew during its first 18 years is the subject of the next few chapters.

Chapter 7. The First Two Directors

[1] *Cong. Rec.*, 81st Cong., 1st Sess., p. A3637.

[2] To Board of Editors, *American Scientist*, Mar. 29, 1961, Chron File.

[3] "Are We Losing the Race for Basic Research Which Supports Industry and Defense?", *Pathfinder News Magazine*, May 7, 1952, p. 25. Miss Doris McCarn, to whom the author is indebted for this oddment, saved one of the unexpurgated copies of this issue of a now-defunct bi-monthly.

[4] Draft ltr. to Chalkley, prepared for ATW's signature, Aug. 30, 1951, with the words "Not sent" written on it; ATW, memo to NSB, Sept. 1, 1951, Chron File.

[5] Personal interview.

[6] In Chron File: ATW to Frederick J. Lawton, Oct. 2, 1952; to the President, Oct. 4, 1957; and to A. Hunter Dupree, May 7, 1963.

[7] In Chron File: LJH to W. D. Watson and A. F. Rasmussen, Jan. 23, 1966; and to Harold Orlans, Jan. 17, 1967.

[8] Memo, Sept. 15, 1960, Chron File.

[9] Unless otherwise attributed, most of the information about Haworth and all of the quotes from him are derived from personal interviews.

[10] LJH to John W. Macy, Jr., Sept. 30, 1966, Chron File.

[11] ATW, memo to Seeger, Feb. 16, 1955, Chron File.

[12] June 29, 1963.

Chapter 8

The Educational and Institutional Programs

"In the advance of science and its application to many practical problems," National Science Board Chairman Conant wrote in NSF's first Annual Report in 1951,[1] "there is no substitute for first-class men. Ten second-rate scientists or engineers cannot do the work of one who is in the first rank. Therefore, if the aims of Congress as set forth in the National Science Foundation Act are to be fulfilled, there must be all over the United States intensive effort to discover latent scientific talent and provide for its adequate development."

During the Foundation's first 18 years Conant's words guided its educational effort—an effort that, beginning with the distribution of 624 fellowships to graduate and postgraduate students in 1952, rapidly burgeoned into an array of programs touching all levels of the American school system from kindergarten up.

To appreciate the spirit of these activities, it is helpful to note the stress that Conant—and the Foundation—put on "first-class" science. A review of the agency's early years, published by a Congressional committee in 1966, noted that "by emphasizing high standards in regard to manpower development, the Foundation placed quality ahead of quantity."[2] In Annual Reports, press releases, and messages to Congress, the agency regularly justified its training programs by reference to the country's need for more scientific and technical personnel. In actual practice, however—to quote the 1966 Congressional review—the Foundation took "no overt steps to attract more students into . . . science and engineering."[3] As early as 1954 Waterman was opposing the suggested creation of a special manpower commission by the President. The Director's argument was that shortages of scientific and technical workers were "confined to a few fields, and likely to improve." Any "dramatic emphasis" on the problem, he wrote, "might lead to oversupplies."[4] In the face of the public clamor for more scientists and engineers that followed the launching of the Soviet Union's first *Sputnik* in 1957, the NSF managers clung to their conviction that the country stood to gain more from a limited number of able and properly trained professionals than from a plethora of inadequate ones.

Time would illustrate the prudence of their strategy. Responsibility for the oversupply of scientists and engineers, reported for some sections of the country in the early 1970's, cannot be laid at the Foundation's door. Figures published in 1972 show that during the preceding 11 years, the period marked by NSF's most comprehensive endeavor to upgrade scientific training in the Nation's schools, the percentage of American students acquiring doctoral degrees in science and engineering remained essentially constant.[5]

Two events of the 1950's contributed to the growth of the Foundation's educational work. One was the release in 1955 of an NSF publication, *Soviet Professional Manpower* by Nicholas De Witt. The significance of De Witt's principal finding—that Russia was producing trained scientists and technical men at an impressive rate—was not lost on Congressional budget makers. Two years later the appearance of *Sputnik I* both confirmed the vigor of Soviet technology and stirred a public demand in this country for more education in the sciences—an outcry that the Foundation interpreted to mean not only more but also better.

The Foundation anticipated *Sputnik*. In 1953 it gave form to the first addition to its educational activities by making possible two summer training sessions for college teachers, one at the University of Minnesota, the other at the University of Colorado.

Months of study preceded this development. No sooner had Harry Kelly taken over as head of the educational directorate than he and his assistants began looking for ways by which the Foundation could help strengthen the teaching of science and engineering in American institutions. In a staff paper drafted in the summer of 1952, Kelly suggested that the Foundation consider giving "limited financial aid to teachers of science" to permit them to spend summers at research centers or special seminars. . . ."[6] By late fall this movement had yielded the preliminary plans for an experimental project which came to be known as the NSF institutes program. Proposals were invited, and in January 1953 the National Science Board approved the issuance of two grants.[7]

One of them enabled the University of Minnesota to conduct a 5-week summer institute for college teachers of physics. The other underwrote an 8-week summer course at the University of Colorado for college mathematics instructors. At Minnesota the Foundation took care of the tuition and fees of all 21 students. At Colorado where enrollment reached 81 in all, 20 received Federal support with the others paying their own way.[8]

Meanwhile, physicist J. W. Buchta of the University of Minnesota had repeatedly urged the Foundation to extend its institutes program to high school teachers of science and mathematics.

To a crusade for this idea, Dr. Buchta brought the experiences of a long career—as chairman of Minnesota's department of physics since 1938, assistant dean of its College of Science, Literature and the Arts since 1954, and editor of *Reviews of Modern Physics* since 1948.

Buchta argued that the place to start bolstering the teaching of science was at the "grass roots"—and that was in the high schools. For years, he reminded Harry Kelly, colleges and universities had been complaining about the insufficient backgrounds in science of incoming freshmen. Plainly the surest way to produce better trained students was to give them better trained teachers. He cited the practice in many secondary institutions of requiring an instructor trained in one aspect of science to teach other aspects for which he had little or no formal training. The provision of appropriate courses for those caught up in these situations, he argued, would improve the quality of science education in high schools all over the country.

Kelly and his associates agreed with the Minnesota educator, but information reaching NSF convinced them that it would be futile to submit Buchta's proposal officially to the National Science Board. Most members of that body were known to be worried by widely expressed opposition to the idea at the country's teacher's colleges. Leaders of those institutions regarded the training of secondary teachers as their special province. Some of them considered its intricacies beyond the competence of the Foundation. Many members of NSB, in addition to a natural reluctance to quarrel with the schools-of-education people, harbored the feeling, as one of them put it, that for NSF to enter the high school field would be "to demean" the organization.[9]

The Foundation's failure to consider the proposal from Buchta did not deter that determined scholar. At the University of Minnesota, during the 1953 summer term, 34 high school teachers heard lectures, watched demonstrations, and took part in discussions dealing with the basic concepts of "Modern Physics"—an institute that Buchta succeeded in setting up with funds from a private endowment.

Foundation authorities watched its progress closely. Obviously they were impressed. In the summer of 1954 the first NSF-funded session for secondary teachers took place at the University of Washington. Within 3 years the bulk of the agency's expenditures for teacher training were going to such institutes, and in 1959 NSF enlarged the program by providing similar sessions for elementary school administrators and teachers concerned with science and mathematics.*[10]

---

* NSF discontinued its institutes for elementary school people in 1966. By this time funds for institutes were spread pretty thin, and in the words of one employee of the educational directorate "something had to give." From 1959 through 1966 the number of elementary school personnel participating in the institutes came to 20,816. (Haworth to Rep. Carl Albert, Jan. 11, 1967, Chron File); NSF *Annual Report*, 1966, p. 110.)

Such were the beginnings of a program that reached its high point in 1968. That year NSF grants totaling $43.8 million permitted 43,612 teachers—most of them from the country's high schools—to participate in 518 summer sessions, 183 academic-year institutes, and some 2,000 less ambitious offerings, including in-service seminars during the regular school term and conferences during the summer recess.[11]

Few of the Foundation's activities have been more popular on the Hill. Even Senators and Representatives uncertain what to think about Government support of some of the more esoteric aspects of scientific research readily comprehended the utility of training sessions for high school teachers, especially when NSF funds for these purposes went to colleges and universities in their home States.

To the leaders of the Foundation, legislative enthusiasm had its disadvantages. When in 1957 Congress ordered NSF to spend "no less than $9½ million" for training high school teachers, Director Waterman, always partial to having a free hand in the dispersal of agency funds, confessed in a letter to a member of the National Science Board that he thought the legislators had acted "unfortunately."[12] Congressional insistence on the widest possible geographical distribution of teacher-training funds loosed sighs at Foundation headquarters. A not infrequent lament in those regions was that too many institutes were being held at "places like the East Bicycle State Teachers College," not enough at colleges and universities known to be strong in science and engineering. The request of one U.S. Representative for a summer session to invigorate the faltering economy of his district found Waterman replying that "the purpose of the NSF-supported institutes is to improve the teaching of science and mathematics. . . . Such a project would seem to bear little relevancy to the purposes served by an Area Redevelopment Bill."[13]

These and other irritations notwithstanding, NSF-funded institutes remained a highly visible feature of the American educational landscape until a combination of events necessitated a considerable cutback in them in the early 1970's.

In a lively account of their progress from 1954 to 1965, Hillier Krieghbaum and Hugh Rawson credit the institutes with helping bring about a material improvement in the teaching of science in American high schools.

"In the long view of history," write the authors of *An Investment in Knowledge*, "possibly the greatest contribution will be that the National Science Foundation institutes . . . helped to focus an evolving philosophy of teacher training on a key idea: That subject-matter courses should receive essential emphasis. The 'workshop' idea that centered around how-to courses has been supplanted by subject-oriented work, such as that given in institutes. This key idea has been adopted not only for other educational areas in the United States but by foreign countries."

Krieghbaum and Rawson add that "while this educational reorientation was taking place, the institutes . . . also played a quiet role in helping integrate some southern colleges and universities." Their reference is to the requirement by the Foundation that colleges and universities desirous of running institutes open them to candidates "solely on the basis of their ability to benefit from the Institute" and "without regard to race, creed, or color." As early as 1963 some southern institutions of higher education began reversing longstanding policies of racial restriction to make themselves eligible for NSF-institutes grants.[14] *

On November 20, 1957, Waterman sent to President Eisenhower a draft copy of the first volume of a four-volume high school physics text. Prepared with NSF funds, the new text was the work of a body of physicists and educators headed by Professor Jerrold R. Zacharias of Massachusetts Institute of Technology and known as the Physical Science Study Committee. Waterman reported that Zacharias' group was also designing teaching aids to supplement its literature. He expressed "high hopes" that the committee's labors would "provide new incentives and enthusiams not only among the teachers of elementary physics in the country but also on the part of their students."

Thus began the Course Content Improvement program, Foundation backing for which would grow during the next decade from $835,000 in 1958 to $20 million in 1968. "If this movement is successful," Waterman wrote following completion of the first NSF-funded physics book in 1957, "it may . . . set a pattern for other scientists to follow in their own way."

It did—with Foundation encouragement. In March 1958 Waterman informed President A. Whitney Griswold of Yale that in line with resolutions passed at two recent conferences of American mathematicians, NSF would entertain a request for a grant "of up to $100,000" for a proposal aimed at the betterment of the Nation's elementary and secondary mathematics curricula. Out of this feeler came the creation at Yale of the School Mathematics Study Group and its production of a new course for grades 4 through 12—events that date the conception and birth of what irreverent journalists were shortly calling "the fun-filled New Math."[15]

---

* The change of policy, to be sure, almost always applied only to the Summer Institute, not to the university as a whole.

Other NSF-supported texts and teaching aids followed. Among those for use at the precollege level were two new approaches to the study of chemistry by the Chemical Bond Approach Project and the Chemical Education Material study, a resource book on topics in geology by the American Geological Institute, and a group of texts, lantern slides, and films on the atmospheric sciences by the American Meteorological Society.[16]

Spurring the development of the Course Content Improvement program was the realization, as set forth in the Foundation's Annual Report in 1958, "that the courses in science and mathematics offered in most secondary schools now reflect neither the current state of knowledge nor the attitudes of mind which characterize modern scientific study." In the 1950's many of the science and mathematics texts being used in the country's high schools were as much as 60 years behind the times.[17]

In its effort to update curricula NSF had to walk the narrow line that separates leadership from dictation. Care was required to avoid actions open to the charge that the Federal Government was telling the Nation's schools what to teach. For several years leaders of summer institutes were forbidden to use Foundation-funded texts and equipment lest participating teachers get the impression that the Government was trying to sneak its materials into their classrooms.

Efforts to stave off such criticisms did not prevent them. On occasion members of Congress heard from citizens convinced that due to the NSF Course Content Improvement program their children were "being educated by Federal civil servants."

In a long letter in 1961,[18] Waterman assured Senator Mike Mansfield of Montana that these fears were groundless. According to the NSF Director, his agency limited its support of new high school courses to their development, production, and testing. Grants were made to bodies of scholars and teachers qualified to study curriculum needs. When a study committee produced acceptable material, NSF allotted funds to cover the preparation of the books for publication and the study group arranged for a number of schools to try out the material. NSF also supported Educational Services, Inc., a nonprofit organization set up as an intermediary among those responsible for preparing the materials, the schools that used them, and the producing publishers and equipment makers.

Items provided with the help of NSF funds, Waterman wrote, were available to "all potential users" but the Foundation "always took the position that it should not promote the use of such materials at the local level."

All grants for underwriting the printing of textbooks carried a provision designed to insure their scientific integrity. With the backing of NSF, the study groups as a rule would not allow editors to delete passages likely to offend elements of the population. At the dictation of sales-minded publishers, some of the older high school biology texts omitted mention of touchy subjects, such as human reproduction and the theory of evolution. Not so the books prepared by the NSF-supported Biological Sciences Curriculum Study.

When in 1964 three BSCS publications won approval from the State Board of Education in Texas, a controversy erupted following the publication of objections by some religious leaders to the inclusion in the texts of the theory of evolution. The flurry in Texas was shortlived, but not quite a decade later the issue resurfaced with a religious journalist bringing a court suit in an attempt to compel federally financed biology texts to give the theory known as creationism "equal time" with evolutionism. NSF resistance to this move reflected the view of its managers that creationism—the doctrine or theory that a transcendent God created matter, life, and the world out of nothing—is a religious concept and, therefore, outside the scope of a scientific publication.[19]

Like the summer institutes for teachers, NSF's labors to improve the content of high school science courses were popular with the educational community. This situation, however, did not insulate them from the slings and arrows to which all Government-financed projects are subject.

Who got the royalties and other receipts earned by NSF-funded books and training aids? That question arose from time to time among interested citizens. The answer was that every contract with a publisher or equipment firm provided for a distribution of the earnings among all of the groups involved, including NSF. Usually the study committee that had developed the material kept some of the earnings for uses prescribed by the Foundation—the production of new editions of the committee's textbooks, for example. American school systems are reluctant about purchasing texts carrying old copyrights. Consequently, publishers have such books revised at frequent intervals. Permission from NSF for a study committee to use a share of the receipts for underwriting revisions sometimes made it unnecessary for the Foundation to authorize a grant for this purpose. As a rule, a portion of the receipts went to the Foundation itself, or more exactly to the Federal Treasury. Dr. Keith R. Kelson, long a top official in the NSF education directorate, recalls that in the case of one widely used chemistry book the Government's share of the earnings amounted to about $1.5 million.

In a letter to Senator Wayne Morse of Oregon in 1964 a disturbed constituent cited a newspaper report that the textbooks prepared by the Biological Sciences Curriculum Study were costing the taxpayer "at least a million dollars" apiece. Asked to comment on this complaint, Director Haworth provided the Senator with a revealing description of the work required to modernize an important high school curriculum. "You are quite right," he wrote the Senator,

> in implying in your letter to [the constituent] that much more was involved in this project than the writing of three textbooks. To date, the project has developed textbooks, laboratory manuals, and teacher's guides for three different high school biology courses. In addition, the project is preparing thirteen so-called 'laboratory blocks' which encourage inquiry in depth into particular major problems of biology and give students an opportunity to find out how biological research is done. For less able students some supplementary materials on difficult topics are being developed. . . . For very bright students a series of real research problems is being published. . . . In addition . . . a series of approximately 50 single-concept films is being prepared. All of the foregoing has been accomplished with the aid of the $5,000,000 mentioned in the newspaper article. The work has involved the collaboration of more than 1,200 biologists, educationists, and teachers. . . .
>
> It may also be noted that more than 3,000,000 students take high school biology each year. Since the materials will clearly provide good models and sources to a significant fraction of these students for some years to come, the total expenditure seems reasonable in comparison with the importance of the problem and the magnitude of the effort needed to effect a major degree of improvement.[20]

Testifying before a congressional committee in the summer of 1965, Haworth mentioned one of the more noticeable effects of the Course Content Improvement Program. Many American colleges, he told the lawmakers, were upgrading their teaching of science because "the youngsters who are coming out of the courses in the high school that use these new [NSF-funded] materials . . . just know too much for the old college textbooks."[21]

Together the Course Content Improvement program and the teacher-training institutes represented the most direct effort by the Foundation, during its first 18 years, to enhance the quality of American education in the sciences. Throughout the same period a slightly different intent was visible in the agency's gradually growing support to individuals in the form of fellowships and traineeships. Here, NSF's principal objective was to make certain that educational opportunities were opened to the country's more talented students and practitioners of science.

Where an organization as young as NSF is concerned, the application of the past tense to any phase of its work is a risky procedure. Still, from the vantage point of this moment (1975) it can be said that the heyday of the Foundation's fellowship and traineeship projects began in the aftermath of *Sputnik* and that they were slowing down when Dr. Haworth retired from the Directorship in the late 1960's.

It will be recalled that the agency started this work in 1952 with modest support for two programs, one for graduates and the other for recent recipients of the Ph. D. degree. The small budgets of those days held the operation to this level until 1956 when the first expansion came with the addition of the Senior Postdoctoral Fellowship program. As the name indicates, the purpose of this activity was to enable even more experienced scientists to engage in a period of advanced study. Most of the 40 senior fellowships distributed in 1956 benefited individuals who had held a Ph. D. degree for at least 5 years. A few went to medical doctors, dentists, and veterinarians interested in further schooling in the basic sciences undergirding their professions.

The next expansion occurred in 1957. Known as the Science Faculty program, it provided fellowships to college teachers in need of additional training. Two years later this opportunity was extended by the establishment of Summer Fellowships for Secondary Teachers of Science and Mathematics—a movement terminated in 1965, by which time enough NSF summer institutes were being conducted on a sequential basis for high school teachers seeking advanced degrees to work for them at these sessions.

Two other innovations reflected the desire of the Foundation to ameliorate difficulties its own awards had created. By 1959 universities were complaining that the giving of fellowships to their better students of the sciences was rendering it difficult for their graduate schools to recruit capable teaching assistants in those fields. Hence, NSF's introduction that year of a program that made graduate teaching assistants eligible for fellowships under which they could work fulltime during the summers on their own study and research. The assumption was that the availability of fellowships to teaching assistants would make those posts more attractive to good students.

Under all of these programs recipients of NSF awards could use them at any school they wished. Most enrolled at the better known centers of scientific training, such as Harvard or California Institute of Technology. Objections to this concentration were soon forthcoming, and in an effort to funnel funds to a greater number of institutions the Foundation in 1959 inaugurated the Cooperative Graduate fellowships.

NSF opened this program to all American universities offering advanced degrees in the sciences, and as the word "cooperative" indicates, the participating schools—105 that first year—took part in the evaluation of applicants. Instead of dealing directly with the Foundation, graduates seeking cooperative fellowships applied for them through their schools. In turn, under a formula previously worked out, the school recommended a certain number of these applicants to NSF, where the actual choices were made. Successful candidates normally had to use their fellowships at the schools from where they applied. Funds for the program, including money to cover the stipends allotted to awardees, went directly to the participating institutions.

When in 1966 the Foundation discontinued cooperative fellowships, it was because a newer NSF activity—the Graduate Traineeship program—appeared to be serving the same ends. A graduate trainee was recommended by his university and obliged to use his award there.

Launched in 1964, the graduate traineeships took their impetus from a survey of the country's scientific and technological manpower requirements by a panel of the President's Science Advisory Committee, a body of consultants whose relations with NSF will be examined in some detail in a later chapter. Released in 1962, the survey was known as the "Gilliland Report" after the chairman of the group responsible for it, Edwin R. Gilliland, professor of chemical engineering at MIT.

A major finding of the report was that one of the Nation's more immediate needs was for "strenuous and sustained efforts to develop more Ph. D.'s in engineering. . . ."[22] For one year, accordingly, NSF limited the issuance of traineeships to graduate students in that field. In 1965 it opened the program to students in the physical sciences and mathematics, and in the following year all NSF-supported sciences were included.

Under other fellowship arrangements, meanwhile, NSF had enabled a number of experienced foreign scientists to study, lecture, and do research at this country's graduate schools. In 1968—the last big year for fellowships and traineeships—the Foundation obligated $45 million for the support of 9,584 awardees. At that point seven such programs were still going. Subsequently, all but one of them were dropped to make way for different types of support for education in the sciences.

The 1960's witnessed the emergence of additional measures to strengthen the scientific component of the Nation's colleges and universities—a group of emprises spoken of collectively as institutional programs.

A problem that one of these entities was intended to rectify had been under scrutiny at the Foundation since its beginning days. When NSF appeared in the early 1950's, the United States Government was spending at least a quarter of a billion dollars a year on grants and contracts to scientists working in the laboratories of schools and other nonprofit institutions.

Already it was apparent that what was sauce for these individuals was not always equally palatable to the institutions where they did their research. An academic biologist endowed with Federal funds, for example, tended to become a center of power in his department, a situation that did not always set well with the administrators responsible for all phases of the department's work. Partly because of the money available to a grantee, partly because of the often uniquely compelling nature of his project, he found it easy to hire bright graduates as research aides—students who might otherwise have worked as teaching assistants.

In the words of Dr. J. Merton England of NSF, the "Government-university relationship was at first one between scientists in the Capital and on the campus, and did not directly involve the university as a whole. But unavoidably, the university did become involved: in bookkeeping and reporting, in arguing over summer salaries, indirect costs, costs-sharing, and expenditure ceilings; in trying to retain control over faculty members' teaching loads; in establishing offices for research administration, which in turn invaded the province of the graduate dean . . .; in keeping up adequate support for investigation outside the mainstream of science, and so on."[23]

Even before NSF got under way, several studies had pointed to the imbalances engendered in the science programs of colleges and universities by the application of Federal money to some aspects of them while the others had to fend for themselves.

At the Foundation, concern over the dilemma arose first in 1952 with a declaration by the National Science Board that NSF should "take the lead" in finding a solution to it.[24] Finding one did not take long. By 1954 or 1955—the records are fuzzy on this point—NSF leaders were giving thought to the plan that would eventually embody their first full-scale attempt to cope with the problem. Several years passed, however, before they acted on it.

The reasons for the delay are spelled out in a diary note drafted by Waterman on July 22, 1959.[25] The Director described the National Science Board as "apprehensive" about the proposed scheme. It involved giving money to colleges and universities on an essentially unrestricted basis. Obviously the schools would have to render accountings. These in turn might provoke "Congressional probes." Nothing in the record suggests that Waterman himself shared these reservations, and a few months later—the plan meanwhile having won approval from the powerful Budget Bureau and the Federal Council for Science and Technology—the National Science Board endorsed it. In this gingerly manner was the Institutional Grants program brought into being.

Its salient features—some of them altered a bit after the program got going—were these: Colleges and universities became eligible for so-called formula grants. Each award was equivalent to a prearranged portion of the sums that, during a certain preceding period, the Foundation (and eventually other Federal agencies as well) had allotted to the school for financing specific research or training projects. The institution was free to use formula-grant money for the benefit of any scientific activity with one exception: None of it could be applied specifically to the costs, indirect or otherwise, of projects already funded by the Government.

Board approval was official by early 1960. In May the Foundation set up an administrative unit, the Office of Institutional Programs, under Louis Levin, a veteran of the senior staff; and a few weeks later an indignant newspaper columnist reported that "a Government-supported organism of cloudy purposes"—meaning NSF—had announced that it was "giving away $2,900,000. . . to various colleges and universities with no strings attached."[26]

Actually, the dispersal of the first formula grants in 1961 wasn't quite that generous—$1.4 million's worth to 248 colleges and universities. By 1968 these figures had risen to $14.2 million and 497 schools. During the first decade of the program approximately one fourth of the Nation's institutions of higher education were beneficiaries.

Annual accountings showed that the grantee institutions spend about one-half of their formula funds on equipment for scientific research and instruction. Other frequently reported uses included the initiation of scientific investigations by faculty members, the purchase of computer time, the procurement of books and periodicals for science libraries, and the payment of fees for visiting lecturers and of stipends to student researchers, technicians, and teaching assistants.

Another institutional program took effect in 1965 with the issuance of the first centers-of-excellence grants. Officially known as the Science Development Program, this movement, like its predecessor, was addressed to an old problem. Government studies harking back to World War II showed that the capacity for providing first-class training in the sciences was limited to a fraction of the country's institutions of higher education. One consequence of this concentration of quality was that Federal support for extramural research gravitated to these few schools. A survey released in the early

1950's indicated that half of the money spent by the Government on university science was going to only 11 institutions.[27] A decade later the situation remained substantially unchanged with 10 universities accounting for 40 percent of such expenditures.[28]

Awareness of this imbalance showed up in NSF's first Annual Report in 1951. Action on it began a decade later, following the publication of a study sponsored by the President's Science Advisory Committee and prepared by a panel under the leadership of Glenn T. Seaborg, then Chancellor of the University of California, Berkeley, later Chairman of the Atomic Energy Commission.

The Seaborg report did not recommend that the Government simply shift its funds to less qualified institutions. "Mediocre research," the authors of the study asserted, "is generally worse than useless, and the same may probably be said of teaching." It followed, in their opinion, that the Government should continue support of "outstanding work" in both fields "wherever it may be found." But as the "existing strong institutions cannot fully meet the Nation's needs," the authors called for a drive to lift the number of "first-rate academic centers of science" from "the fifteen or twenty" existing in 1960 to twice that many by 1975.[29]

It was in response to this plea that NSF, early in 1961, started casting about for ways of diversifying the availability of quality training in the sciences by developing additional centers of excellence throughout the country. NSF staff members visited 12 colleges and universities to appraise the status and needs of their science and engineering departments. Ad hoc study groups within and without the agency weighed a variety of schemes.

As approved by the National Science Board in the fall of 1962, the Science Development Program contemplated grants large enough to enable the schools which qualified for them to upgrade their research and instructional activities over a period of 5 years. It was understood that none of the "fifteen or twenty" top-rated institutions need apply. When one of them did, its president received a polite note from Director Haworth, reminding him that development grants were strictly for the second-stringers.[30] Two major criteria guided NSF in the selection of grantees: A school had to demonstrate a potential, as Haworth wrote, "for taking a major step forward to a position among the leaders . . ."[31] In addition, it had to show the availability of funds, over and above those received from NSF, as assurance of its ability to maintain a high level position once that was achieved.

NSF's original plan was to start the Science Development program "in an experimental and exploratory manner" in 1963.[32] But this was not to happen. Considerable time was required to devise guidelines for grants that would range from $3 million to $5 million each. Not until the summer of that year was the Foundation ready to ask for the necessary money; and at this point a Congress, ordinarily delighted with any effort by the agency to broaden the geographical distribution of its funds, turned out to be more interested in economy. Both branches turned down a request of $30 million to put the grants under way. In the House, Albert Thomas' appropriations subcommittee went further. It forbade the Foundation to start any new programs at all during the forthcoming fiscal year.

The development grants were not the only ones to be caught in this bind. In the fall of 1963, when the ban on additional programs appeared, NSF's educational directorate was getting ready to issue the first graduate traineeships. Staff member Francis G. O'Brien was already on the road, meeting with groups of graduate deans for the purpose of familiarizing them with NSF's plans and of giving them a head start on the preparation of proposals.

Early in October the New England deans gathered at a hotel in Boston, and on reaching the meeting room one morning, O'Brien found on his desk a marked copy of the *New York Times*,[33] carrying the report that Congress had ruled out all new NSF programs. What did the assembled educators do about that? "We just went ahead with our meeting," says O'Brien, "and hoped for the best."

The hopes were not misplaced. Early in January Haworth was conveying the good news to Harvey Brooks, Dean of Engineering and Applied Physics at Harvard and member of the National Science Board. The Director had arranged a little "talk with Albert Thomas and others" on the Hill. Apparently the Congressmen had bowed to Haworth's argument that the traineeships and the development awards should go forward because both had been planned at the request of the White House. In any event, the Foundation now had permission to institute the two new programs on a modest basis.[34]

During the last week of March 1964 the presidents of all eligible academic institutions in the United States, its territories, and possessions were invited to submit plans for enhancing the quality of their science and engineering departments. A hundred and fifty of them did so, and on May 4, 1965, President Lyndon B. Johnson announced the award of the first four centers-of-excellence grants to the University of Oregon, Washington University (St. Louis), Case Institute of Technology (Cleveland), and Western Reserve University (also Cleveland).

By 1968 the Foundation was operating five institutional activities in all. Two of them were expansions of the development program—one designed to help improve individual science departments, the other aimed at the special needs of 4-year colleges.[*] Under another arrangement, NSF funds, contributed on a matching basis, had enabled hundreds of graduate schools to modernize and enlarge their scientific plants.

Of the five programs only one would survive the changes of the next 6 years. But about these developments, more later. At this point other aspects of the growth of the Foundation under its first two directors demand attention.

---

[*] NSF was not the only Federal agency to inaugurate science-improvement programs on an institutional basis during the 1960's. Among the somewhat similar activities operated by other agencies were the Health Sciences Advancement Awards of the National Institutes of Health and the University Sustaining Grant Program of the National Aeronautics and Space Administration. (Louis Levin, "The Federal Thrust to Improve Institutional Quality: A Retrospective Look," speech before the 139th annual meeting of the American Association for the Advancement of Science, Washington, D.C., Dec. 27, 1972.)

Chapter 8. The Educational and Institutional Programs

[1] P. vi.

[2] *The National Science Foundation: A General Review of its First 15 Years*, 89th Cong., 2d Sess., Washington, D.C., 1966, p. 125.

[3] Ibid.

[4] ATW, diary note, Oct. 21, 1954, Chron File.

[5] NSF, *Scientific Human Resources: Profiles and Issues*, Washington, 1972, p. 3.

[6] Hillier Krieghbaum and Hugh Rawson, *An Investment in Knowledge: The First Dozen Years of the National Science Foundation's Summer Institutes Programs . . . 1954-65*, New York, 1969, p. 88 (cited hereafter as Krieghbaum).

[7] NSB Min. 18:9.

[8] Krieghbaum, pp. 102, 120.

[9] Ibid., pp. 90-100.

[10] Ibid., pp. 101-104; AR 1955, p. 70.

[11] AR. 1968, pp. 162, 178.

[12] ATW to Keith Glennan, Aug. 31, 1957, Chron File.

[13] To Rep. John A. Blatnik, Mar. 7, 1961, Chron File.

[14] P. 333.

[15] The letters cited are in Chron File; that to Griswold is dated Mar. 11.

[16] ATW to Sen. Mike Mansfield, Jan. 11, 1961, Chron File.

[17] AR 1958, p. 64.

[18] See note. 16.

[19] Interview with NSF officials. For a recent discussion of the creation-evolution argument, see "Creationism," *Scientific American*, January 1971, pp. 46-47.

[20] Oct. 22, 1964, Chron File.

[21] *Review of the National Science Foundation*, Hearings Before the Subcommittee on Science Research and Development of the [House] Committee on Science and Astronautics . . ., 89th Cong., 1st Sess., Washington, 1965, vol. I, p. 44. (Cited hereafter as Review Hearings I.)

[22] PSAC, *Meeting Manpower Needs in Science and Technology*, Washington, Dec. 12, 1962, pp. 30-31.

[23] "Interesting Times—The NSF Since 1950," *Mosaic*, Winter 1970, p. 5.

[24] NSB Min. 16:7.

[25] Chron File.

[26] Clipping of an unidentified newspaper column in ltr. from a constituent to Sen. Hugh Scott, n.d., in Chron File, July 1960.

[27] Cited in AR 1950-51, p. 16.

[28] "White House Superstructure for Science," *Chemical and Engineering News*, Oct. 19, 1964, p. 92.

[29] PSAC, *Scientific Progress, the Universities, and the Federal Government*, Washington, Nov. 15, 1960, p. 14.

[30] LJH to Lee A. DuBridge, Feb. 1, 1968, Chron File.

[31] LJH to Sen. E. L. Bartlett, Oct. 23, 1963, Chron File.

[32] ATW, memo to NSB, June 19, 1962, Chron File.

[33] Oct. 8, 1963.

[34] LJH to Brooks, Jan. 3, 1964, Chron File.

Chapter 9

Larger Ventures

During those first 18 years most of the Foundation's programs originated within the agency. As has already been noted, however, the stimulus for several of them came from the outside—some being instigated at the request of other organizations, others as the result of legislative or executive action.

It was at the urging of the National Academy of Sciences, for example, that in 1955 the Foundation began administering the funds—$43.5 million in all—sanctioned by Congress to help finance this country's participation in the International Geophysical Year. As the term itself shows, the IGY was a worldwide venture. For 18 months beginning July 1, 1957, some 30,000 scientists and technicians from 66 nations cooperated in a study of planet Earth. One of the larger programs begun during the IGY—a multinational exploration of the Antarctic (later extended to the Arctic)—still enjoys extensive support from the Foundation.

In 1958, to mention another example, Congress passed a law that required NSF to manage the Federal Government's research programs on weather modification. The House Appropriations Subcommittee chairman, Albert Thomas, was not enamored of this development. The Foundation's charter forbade it to manage anything but its own internal affairs. But "Here," Thomas complained at a 1960 budget hearing, "is a specific job dumped on you."[1] Unmoved by the chairman's grumbles, NSF proceeded to oversee and coordinate the Government's weather-control activities until the late 1960's when Congress transferred most of these supervisory duties to the Department of Commerce.

NSF's plunge into this area was not without its light moments.* The spring of 1966 brought to Foundation headquarters a letter from the Rain Dance Chairman of the Chamber of Commerce in Prosser, Washington, The chairman wrote:

> We have been told that a new regulation requires that "any persons planning to attempt a modification of the weather"

---

* There were also dangerous moments. In at least two states members of groups opposed to what they called "tampering with the weather" shot at planes sent up for that purpose. (*National Science Policy*, Hearings before House Subcommittee on Science, Research and Development, 1970, pp. 223-24.)

> must first register with your office, or be subject to a $500 fine.
>
> Prosser Chamber of Commerce has scheduled a rain dance for . . . May 20. . . . Please consider yourself advised.
>
> This is the third year. . . that Prosser has scheduled a rain dance. . . . The two rainstorms we danced up in past years were greatly appreciated by the wheat farmers in this parched area. . . . Our method of shaking loose the clouds involved about a half-dozen Yakima Indians, doing traditional rain dances, and several hundred teenagers doing more modern variations—the jerk, frug, alligator, and so forth. The latter, we believe, is responsible for the cloud-bursting success.

If P.H. Wyckoff, the NSF program director for weather modification, suspected a put-on, he gave no indication of it in his reply to the Prosser rainmakers. He told them that the "notification of intent" regulation did not apply to the type of weather modification techniques they were planning to use. "Rain dances," he recalled, "have been going on for centuries . . . and it is our feeling that (they) . . . have become a normal input to the weather pattern in much the same way as lunar tides, atmospheric dust, and meteoritic particles which are also believed to influence normal weather patterns." He wished the rainmakers luck, and expressed the hope that should their ceremony produce any scientific phenomena, they would so inform the Foundation.[2]

On September 2, 1958, the signing of the National Defense Education Act by President Eisenhower had the result of enlarging the Foundation's attempts to carry out a directive in its enabling act "to foster the interchange of scientific information among scientists in the United States and foreign countries." In accordance with the 1958 law, supplemented by an executive order in 1959, NSF dismantled a small information unit and replaced it by a considerably expanded Office of Science Information Service. The Foundation's subsequent provision of funds for the preparation at Princeton University of techniques was one of many programs developed to improve the indexing, automatic and computed indexing/abstracting, translating and other publishing and retrieval services involved in the propagation of scientific information.

Even if the Foundation were not bound by law to encourage the diffusion of data throughout the world, it could scarcely fulfill its obligations without doing so. The dissemination of information is as vital to science as up-to-date equipment. "The whole procedure of publication and citation," writes the British theoretical physicist, John M. Ziman, ". . . the libraries full of periodicals and treatises . . .

freedom to learn and freedom to teach—cosmopolitanism and internationalism, conferences, abstract journals and encyclopedias—all are in the service of the mutual exchange of information. If there is . . . a technical crisis in these procedures, if . . . the highways can no longer carry the traffic of ideas. . .then Science itself will be severely hampered."[3]

Another directive from Congress came in 1966 with the passage of the National Sea Grant College and Program Act. Obedient to this legislation the Foundation, beginning in September 1967, obligated millions of dollars annually to underwrite research and development, educational programs, and advisory services to enhance the Nation's efforts to augment and use the resources of the sea. Much of this support took the form of block grants to American universities having sufficient equipment and expertise to pursue large-scale marine projects ranging from a study of ocean law to the application of acoustical techniques to the location of food and minerals under the waves.

Even before the Foundation got around to distributing its first project grants for basic research in 1952, it had signalled its intention of earmarking some of its funds to help colleges and universities obtain scientific equipment and facilities beyond the reach of their budgets. What imparts interest to this early action is that no specific authorization for it existed in the agency's charter. All that stood behind it was the conviction of the NSF leaders that their mandate "to promote the progress of science" implied the power to underwrite the planning, purchase, and fabrication of necessary facilities. True, there were precedents. On occasion, since World War II, the Department of Defense, the Atomic Energy Commission, and the National Advisory Committee on Aeronautics had financed large research facilities related to their missions.

More to the point, Congress had no objections. This became clear in the summer of 1956 when the Foundation asked the legislators for enough money to begin construction of the big telescopes, laboratories, and other facilities that now comprise the National Radio Astronomy Observatory, a 2,750-acre research center near Green Bank, W. Va. Congress cut this request from $7 million to $3.5 million, but neither branch questioned the right of the agency to go ahead with its plans.

To appreciate NSF's interest in radio astronomy in the 1950's calls for a glance at the beginnings of this technique for exploring the skies. Because the stars are millions of miles away investigators derive most of their knowledge of them from a study of the energy these distant objects radiate in the direction of Earth. For centuries almost the only source of such information lay in light waves from stars that are not absorbed by the terrestrial atmosphere and can be picked up by ground-based optical telescopes.*

Then came radio astronomy. This new branch of an ancient science grew out of the late nineteenth-century discoveries and inventions that produced first radio and then television broadcasting. During the early days of radio, reception was often marred by extraneous noises. To the owner of a receiver purchased for entertainment this static was merely annoying. To men dependent on "wireless" communication across the ocean, from ship to ship or between ship and shore, it could be a serious problem.

In the early 1930's, therefore, Bell Telephone Laboratories commissioned a young engineer, Karl Jansky, to investigate the origin of static as a step toward its possible elimination. Jansky set up a rotating antenna on an abandoned potato farm in New Jersey—and in 1932 he was able to report that much of the unwanted racket was coming from the Milky Way. Somewhere in that enormous galaxy to which our own solar system belongs was a source of radio waves.

Once Jansky had ascertained that the cause of static was in the stars and beyond human reach, Bell Telephone terminated his experiments. Fortunately for science, his published reports prompted another young engineer, Grote Reber, to erect in his backyard in Wheaton, Ill., a radio telescope in the form of a paraboloid saucer. With a diameter of 31½ feet, the dish built by Reber was so mounted that he could aim it at any part of the sky available to him just as an optical telescope can be directed. First in Wheaton, later in other parts of the world, Reber succeeded in pinpointing some of the celestial sources of radio noise.

Jansky and Reber thus brought into existence a new way of studying the sky, a new window onto the universe. At first astronomers paid little attention to their work, but this situation changed as the result of developments during World War II. One of these, a byproduct of the extensive use of radar in that struggle, was the invention of electronic equipment with receivers much more sensitive than those available to Jansky and Reber. Another was the prediction by a scientist in the Netherlands (based on theoretical considerations subsequently confirmed at Harvard) that with such equipment scientists could observe very low wavelengths in the radio-wave portion of stellar and solar spectra, thus learning a great deal about the chemical composition of space and the structure and behavior of the objects in it.

---

*With the emergence of the space age after World War II astronomers' opportunities for examining the heavens have been enlarged by the ability of research rockets and satellites to carry their instruments to points beyond the obscuring atmosphere of Earth.

By the early 1950's radio astronomy was beginning to come into its own. England had started building the now famous steerable paraboloid scope, 250 feet in diameter, at Jodrell Bank. In the Netherlands and Australia somewhat smaller but highly sensitive radio telescopes were in the design or construction stage.

In the United States the Naval Research Laboratory was operating a 50-foot scope and a few universities were planning instruments of similar size and capacity. On the whole, however, progress was so halting as to raise the feeling at the National Science Foundation that here was a field where the Government would be justified in making a large investment if only to put the country whose engineers had pioneered the technique on a par with other nations or ahead of them.[4]

Aware that they were embarking on their first venture into "big science"* the Foundation leaders moved slowly. The National Science Board had reservations. Its view of the matter was marked by members' concern at what appeared to be the tendency of big science to crowd out little science. In a carefully worded resolution they said that any expenditures for capital facilities must come from funds over and above those needed for the project-grants program. Nothing, in short, was to interfere with NSF's extension of assistance to individual investigators.[5]

With this admonition in mind the Foundation put in train the labors that would bring into being the first of the five national research centers now run with NSF funds.

Approximately 3 years of study and planning preceded the decision in 1956 to ask Congress for the necessary starting funds. The law forbids the Foundation to operate a laboratory and a contract for this purpose was negotiated with Associated Universities, Inc. Under its terms AUI assumed primary responsibility for both the establishment and management of the center.

In the beginning a few questions arose concerning the arrangement. Speaking as chairman of a panel set up by NSF to study the feasibility of a national laboratory for radio astronomy, physicist Merle A. Tuve pointed out that AUI, essentially a nuclear science organization, had no experience in the field.[6] From other sources came the objection that all of the universities composing the incorporated consortium were in the East and that it was of the essence that the facilities of the center be available to investigators from every section of the country.

---

* What is big science? Asked that question by a congressional committee in 1969, Grote Reber put the dividing line at a million dollars. Anything costing less was little science. (*Independent Offices . . . for Fiscal Year 1970, Hearings before Subcommittee of the* [Senate] *Committee on Appropriations,* Washington, 1969, p. 728.)

Those worries fell before other considerations. Waterman expressed complete faith in AUI. He cited the corporation's successful operation of the Brookhaven National Laboratory on Long Island under a contract with the Atomic Energy Commission. "The management of the large sum of money involved," he argued, "is usually best handled by picking an existing organization that already works as a team and has wide experience."[7] Another factor in the decision of the Foundation to use AUI was the great enthusiasm for the radio astronomy project on the part of the corporation's respected president, Lloyd Berkner.

By spring 1956 plans for the observatory were well advanced and the site near Green Bank, W. Va., had been decided upon. To a large extent the selection of this area, a triangular valley ringed by mountains, was dictated by its remoteness from any heavily settled region. Radio telescopes must be as free as possible from manmade radio noise. In August, at the request of Waterman, the West Virginia legislature passed a law restricting the use of electrical equipment within a 10-mile radius of any radio astronomy facility in the State.[8]

Notwithstanding these early attempts to prevent interference, the land originally acquired by the Government turned out to be somewhat inadequate in this respect. As the first telescopes to be finished went into action, a 10-acre farm on the outskirts of the center presented a problem. Its lone occupant was an old widower, and the coming and going of cars when members of his family came to call troubled the sensitive antennae at the observatory. At first the old man refused to sell. Eventually, however, he was persuaded to do so by the argument of his relatives that a man with an ailing heart had no business spending the hard West Virginia winters all by himself on an isolated farm.

On another occasion the managers of the observatory deeded away some of their property. They did this on being informed that the local community needed space for an overcrowded cemetery adjoining the laboratory area. The community's argument was that "the occupants of the cemetery won't bother you none."[9]

Generally speaking, the facilities originally projected for the center went up on schedule. Construction of the most ambitious of them, however—a high-precision radio-wave detector with a solid-surface paraboloid reflector, 140 feet in diameter—encountered so many snags that 4 years after work on the instrument began Waterman was expressing the fear that it might not be "possible of assembly."[10]

To assess the dimensions of the problem requires a look at the big telescope as it finally materialized after 7 years of starts and stops and changes of plan. The 140-foot paraboloid at Green Bank is the world's largest equatorially mounted device of its kind. Worth noting, however, is that size is not its most significant feature. Green Bank has a much larger telescope, but whereas this larger dish—300 feet in diameter—can move only up and down, the 140-footer is completely steerable. This maneuverability, along with an aperture of efficiency of 43 percent at 1.9 centimeters of wavelength and a pointing accuracy of better than 10 seconds of arc, make it one of the world's most efficient machines for tracking and observing celestial objects.

Its physical proportions are impressive. The pedestal underneath the major working parts rests on rock 30 feet below ground. It rises above ground another 30 feet, has walls 3 feet thick, and contains 5,700 tons of concrete and 140 tons of reinforcing steel. The basement of this structure houses the hydraulic and electrical primary components. Among the facilities occupying the three floors above are the operating console and an electronic workshop.

Atop the pedestal a welded steel base supports the entire 2,700-ton weight of the telescope as transmitted by the main hydrostatic bearings. The structural heart of the instrument is a 400-ton spherical bearing. More or less hemispheric in shape and 22 feet in diameter, this element floats on a film of oil only 0.005 inch thick. Other major components are the 555-ton polar axis (55 feet long and 12 feet in diameter), the 1,212-ton yoke (17 subassemblies welded together), and the mammoth superstructure that supports the 60 panels of the 140-foot reflector and its antenna feed.[11]

In 1957, on the basis of an overall design prepared by Ned L. Ashton, a consultant to AUI, the corporation solicited bids and arranged for the E.W. Bliss Company of Canton, Ohio, to act as prime contractor, responsible for the fabrication of the instrument and its installation at Green Bank. Within a few months after the signing of the contract on June 9, 1958, it was clear that the delivery date specified in it—"approximately December 1960"—was not going to be met. Contemporary accounts in the press spoke of "engineering problems." Today men closely linked to the creation of the 140-foot scope dismiss that phrase as a euphemism.

The problems were basically managerial. Accustomed to the relatively undemanding supervision connected with the distribution of project grants for basic research, the NSF leaders were slow about exercising the much closer monitoring that a multimillion-dollar contract with industry required. At the other end of the partnership, AUI's efforts to oversee the work with a small staff in New York City was marked in the beginning by uncertainty and laxity.

The contract ordered the Bliss company to submit time schedules covering the work it expected to perform during certain stages of the job. When over a period of many months Bliss turned in only one such report, and that on an informal basis, the corporation managers let the matter ride. By this and other concessions they made it difficult for themselves later on to enforce several aspects of their agreement with the Ohio Company. The contract assigned design work to Bliss, but after AUI hired its own consultant to draft overall plans, the company took the position that it had no responsibility for this phase of the work. A conference between Berkner and Robert Potter, president of Bliss, called for the purpose of dealing with this issue, left it as much in the air as ever.[12]

As troubles accumulated Waterman confessed to a member of the National Science Board that "mistakes have been made."[13] In a letter to another member, the Director struck a whimsical note. "In operating under the Government," he told Warren Weaver of the Rockefeller Foundation,

> it has amused me from time to time in budget procurement to see a social analogue of the conservation of energy principle. The sums requested, say for capital facilities, are large. For an individual or a private group to acquire capital for any purpose requires the expenditure of a considerable amount of energy, commensurate with the magnitude of the sum required. Opportunities for acquiring large sums from the Government are most inviting. But according to the First Law,[*] this cannot be easy and calls for the expenditure of corresponding energy. The time, effort, and grief a Government agency puts into the job is the kind of compensation nature demands.[14]

In 1959 nature upped her demands, following an incident in Australia. Primarily press builders and steelmill rolling machine manufacturers, the Bliss Company had come out of the second World War with a large stock of heavy section, low-carbon steel on hand. They were using this "shipplate" in critical components of the 140-foot telescope, and in the closing months of the year a report reached the United States that an Australian bridge built of the same material had suddenly collapsed. Simultaneously, engineers in this country recalled that during World War II a number of American ships, also made of this kind of steel, had mysteriously broken in two at sea.[15]

Disturbed by these reports and rumors, the National Science Foundation set up an investigatory committee under Dr. A. B. (Gus) Kinzel, one of the country's leading research metallurgists. The conclusions of the Kinzel panel confirmed the Foundation's fears. The low carbon steel that Bliss was using in the big equatorial scope was subject to a phenomenon known as brittle fracture.[16]

---

[*] Meaning apparently the First Law of Thermodynamics, formulated by the English physicist, James Prescott Joule (1818-89), who in a paper read before a scientific gathering in 1843 voiced the conviction that "whenever mechanical force is expended an exact equivalent of heat is always obtained."

What this means, to quote an AUI engineer, "is that shipplate tends to split at the weldments when exposed to even rather small changes of temperature. Well, the temperature at Green Bank, in the course of a day, has been known to swing from 50 degrees down to a minus 10, about a 60-degree effect. You can protect an optical telescope by putting it under a radome. You can't do that with this radio telescope [meaning the 140-footer]. It has to stand out there, open to the elements, and take the gaff."[17]

Could the 2,000-plus tons of shipplate already worked into the 140-foot telescope take it? If not, what should be done?

At a series of conferences in 1960 representatives of the Corporation and NSF discussed these questions at length. One thing was uncomfortably clear. AUI's experts had approved of the steel that the Ohio manufacturers were using. Any expenditures for necessary changes must come from the Government. The Bliss people had taken care to point out these facts, having first consulted their own metallurgists. Meanwhile, work on the 140-foot telescope had come to a halt.

So matters stood when in February 1961 the Foundation and AUI took what would prove to be another wrong turn. Both the agency and the corporation had already expressed dissatisfaction with aspects of the Bliss Company's performance. Instead of breaking off with the company at this time, however, they tried to patch things up by altering the arrangement under which it was doing the work.

This arrangement was a so-called turnkey contract that called for the Ohio firm to fabricate and erect the 140-foot telescope for a lump sum of $4,750,000. Effective February 1, 1961, this document was superseded by a cost-plus-fixed-fee contract under which Bliss was to receive $142,000 in compensation with AUI taking care of all costs.

The original contract stated that "time was not of the essence," a clause that both AUI and the Foundation had come to regard as "unfortunate." The new contract made time of the essence by obliging the Ohio company to complete the work "on or before December 31, 1963." In addition, Associated Universities assumed sole responsibility for design, and provision was made for observers from NSF to sit in on any conferences or negotiations required as the work progressed.

In a long letter drafted to justify these steps, the AUI managers described their original arrangement with Bliss as a serious mistake. Experience had shown them that under the confining terms of a lump-sum contract no manufacturer could be expected to produce so novel an instrument as the 140-foot telescope. The corporation managers pointed out that in setting up the first contract the parties to it "had negotiated as though they were dealing with a device requiring only conventional materials and standards of fabrication and erection." It was now plain that the construction of the 140-footer "should have been regarded as an engineering research and development effort." Where several of the components were concerned, they noted, the "acceptance specifications were hard to attain, e.g., the rigidity of the reflector and the spherical surface." The use "of an equatorial, or polar, mount on such a large structure in itself," they added, "presents unusually difficult problems."[18]

Under the second contract work was resumed at once, but not for long. Although the all-welded aluminum support for the 140-foot reflector was the largest such weldment ever attempted,[19] Bliss brought it off successfully. Other components fared equally well, but some of them continued to give trouble—and in December 1961 the second contract with the Ohio company was terminated "for the convenience of the Government."[20]

At this point Associated Universities turned the job over to Maxwell M. Small, a marine engineer long connected with the corporation. A hearty man, direct and forceful, Small was a logical choice. He was then AUI's business manager at Brookhaven National Laboratory, a position in which he had frequently exhibited extraordinary talents as a trouble shooter.

Talking today in the office he occupies as Special Projects Manager in the Applied Science Department at Brookhaven, Small reconstructs with relish the circumstances under which he agreed to become a one-man prime contractor for completion of the 140-foot telescope.

"I told the corporation I'd try," he says, "on one condition. I was to be given complete responsibility and authority without limit. When they said okay to that, I put together a team and we got going."

AUI had retained the well known Boston firm of architectural engineers, Stone and Webster, to prepare new designs as needed, and early in 1962 Small moved into their offices to supervise this work.

"Being an outsider," he says, "I wasn't the most popular person on the premises. We had some arguments. Naturally Stone and Webster wanted to redesign the telescope from the bottom up, and naturally I didn't want them to. For one thing, Ashton [AUI's architectural consultant] and Bliss had done a good job on many of the components. For another, I wanted to keep costs at a reasonable figure. I remember saying to the Stone and Webster gang, 'Come on, boys, let's not Rube Goldberg everything in sight. Don't redesign something just because it isn't your lines on a piece of paper.' I won some of those battles. They won others."

Several components had to be replanned and remade, notably the huge polar axis and the yoke. What became of the steel that Bliss had put into these elements?

"Sold for scrap," says Small. "Some of it came here"—meaning to Brookhaven, where it now serves as shielding for the accelerators used at the big nuclear science laboratory for the study of charged particles in motion.

When Stone and Webster's designs were finished, Small discharged the Boston engineers and paid them off. He followed this practice with the companies employed to fabricate the axis, the yoke, and their auxiliary parts. Years later Gerald A. Greenwood, an NSF contracts officer, would remember Small's saying that he was "tempted" to keep Stone and Webster on for the duration because even after the primary designs were done, a lot of detailed engineering drawings had to be made. "But somehow," Small said, "I couldn't see paying a company for any work that I could do myself."

It is hardly surprising, given these economical methods, to learn that when the telescope was at last in position and ready to work, about $400,000 of the funds allocated to its construction and installation remained unspent.

In February 1963 Small broke a leg while ice skating on one of the Atlantic bays near his Long Island home. For 9 months he directed the work from a wheel chair, the injured limb in a heavy cast. He kept mobile by having the Brookhaven engineers rig up a car with an accelerator and brakes that he could manipulate with his good right leg.

"For a while," he recalls, "that enabled me to go back and forth—from Brookhaven to Green Bank, or to the factories where the stuff was being made."

Later AUI provided him with a plane and pilot. Still later he moved with his family to Green Bank to be on hand as the shop-made subsystems arrived by railroad and flat-truck and two huge derricks—each 167 feet tall—hoisted them into place.

In February 1965, the 140-foot telescope stood complete—ready for testing and for the dedication ceremonies a few months later on October 13. Its total cost, $14 million, included $4,696,986 to cover the work done by the original prime contractor—and Small devoted the leftover $400,000 to the purchase of electronic refinements for the big instrument after it went into operation.

No seriously untoward incidents marred the establishment of the other national research centers now financed by the Foundation, except that the $6.5 million laboratory building at the National Center for Atmospheric Research (NCAR) in Boulder, Colo., was not yet dedicated—an event that took place on May 10, 1967—when its occupants found themselves coping with a leaking roof. NCAR is operated by the University Corporation for Atmospheric Research, made up of 31 universities with graduate programs in the atmospheric sciences or related fields.

In addition to NCAR and the observatory at Green Bank, the large NSF-supported centers and a few facts about them are:

Kitt Peak National Observatory, on a 6,875-foot mountain 53 road miles southwest of the city limits of Tucson, Ariz.—a site chosen after a 3-year survey of more than 150 United States mountain ranges—is devoted to studies of the sky by both optical and radio means. Under a contract executed in December 1957, the observatory is operated by the Association of Universities for Research in Astronomy (AURA), a non-profit corporation whose members are the University of California, the Universities of Chicago and Texas, Harvard, Indiana University, the University of Michigan, the Ohio State University, Princeton, the University of Wisconsin, and Yale. One of the center's facilities, the Robert R. McMath Solar Telescope, is the largest optical device ever built for exploring the nature and activities of the sun. Its two 158-inch telescopes are equipped with devices sufficiently sensitive to permit observation of very faint stars and distant galaxies. The center has a resident staff and its facilities are used also by visiting scientists and graduate students.

NSF leases the Kitt Peak site from the Papago Indians on whose 2.7-million-acre reservation it stands. Some 7,500 members of the tribe live in the little villages that dot the vicinity. Among the speakers at the dedication of the center on March 15, 1960, was NSF Director Waterman. A few days later he was writing Dr. Laurence M. Gould, president of Carleton College in Northfield, Minn., that he was "touched by an incident after the ceremonies when Mr. Enos Francisco, the Tribal Chief, came up to me and said . . . that he was much moved by the studies that the scientists were contemplating of the heavens to learn more about the universe since this was much the point of view that his tribe had always had. He then gave me to understand that he hoped the work done on Kitt Peak and its association with his tribe would encourage his people to become better educated, especially in science."[21]

Cerro Tololo Inter-American Observatory, also run by AURA, occupies a 7,200-foot elevation in the foothills of the Chilean Andes and was created in 1963 to provide both United States and South American astronomers with a modern optical center for the observation of the southern sky. The Republic of Chile cooperated in the survey that preceded the selection of the site, and the facilities include a 158-inch telescope jointly funded by NSF and the Ford Foundation. Like Kitt Peak the observatory has a resident staff and is also used by visiting scientists. The Cerro Tololo site is owned by the academic corporation that operates the observatory.[22]

National Astronomy and Ionosphere Center, approximately 12 miles south of Arecibo, Puerto Rico, is the home of the world's largest radio-radar telescope, a fixed bowl-shaped reflector, 1,000 feet in diameter, equipped with a movable feed that is suspended by three mammoth towers 435 feet above the reflector. Operated by Cornell University scientists, the Arecibo dish was funded by the Department of Defense from 1963 to 1969, since then by NSF. Use of the facility is open to scientists on a worldwide basis.

Chapter 9. Larger Ventures

[1] *The National Science Foundation: A General Review of its First 15 Years*, 89th Cong., 2d Sess., Washington, D.C., 1966, p. 81.

[2] Bert Johnson and Don Carter to NSF, May 16, 1966; Wyckoff to Carter, June 3, Chron File.

[3] Ziman, *Public Knowledge: An Essay Concerning the Social Dimension of Science*, Cambridge, England, 1968, pp. 143-144.

[4] Richard M. Emberson, "National Radio Astronomy Observatory: The Early History and Development . . .," *Science*, Nov. 13, 1959, pp. 1307-1309; R. Hanbury Brown and A.C.B. Lovell, *The Exploration of Space by Radio*, New York, 1958, pp. 1-2, 114-20; James S. Pickering, *Windows to Space*, Boston, 1967, pp. 131-52; Samuel Glasstone, *Sourcebook on the Space Sciences*, Princeton, N.J., 1965, pp. 355, 873.

[5] NSB Min. 34:10-11.

[6] ATW, Diary Note, Sept. 7, 1954, Chron File.

[7] ATW to J.C. Morris, July 26, 1956, Chron File.

[8] ATW, Memo to NSB, Aug. 15, 1956; Emberson, ibid., p. 1312.

[9] Interviews with NSF officials.

[10] ATW to Earnest H. Volwiler, July 11, 1961, Chron File.

[11] Maxwell M. Small, "The New 140-foot Radio Telescope," *Sky and Telescope*, Nov. 1965, pp. 267-274. (Cited hereafter as Small.)

[12] Record of Meeting (NSF and AUI-NRAO), Feb. 25, 1960, OGC files.

[13] See note 10.

[14] ATW to Weaver, April 7, 1958, Chron File.

[15] Personal interview with Maxwell M. Small.

[16] NSB Min. 68, 70 and 71; ATW to Roger Adams, July 11, 1961, Chron File; Small, p. 268.

[17] Interview with Small.

[18] Memo, Asst. Dir. for Administration to Deputy General Counsel and Grants Administrator: Justification of Contractual Arrangements—AUI and E.W. Bliss Company, Mar. 15, 1961, OGC files.

[19] Small, p. 268.

[20] ATW, Memo to NSB, May 9, 1962, Chron File.

[21] Mar. 29, 1960, Chron File.

[22] LJH to Donald F. Hornig, Aug. 30, 1967, Chron File.

Chapter 10

Tempest in a Samovar

It would appear from a check of American news media for the late 1950's that the managerial lapses connected with the construction of the 140-foot telescope for the radio astronomy observatory at Green Bank escaped the attention of the press. The oversight was perhaps beginner's luck, for in the 1960's other facets of the Foundation's work gave rise to considerable unfavorable publicity.

Responsible for some of this criticism was a stressful incident commonly referred to as the "Smale Affair."

A few facts by way of brushing in the background of this event:

In 1966, when the "affair" began accumulating headlines and sharp inquiries from Congress, its central figure, Dr. Stephen Smale, was in his third year as a professor of mathematics at the University of California at Berkeley. Smale is often spoken of as a topologist because of his extensive work in that field, and at 36—his age in 1966—he was already regarded by his peers as one of the best mathematicians in the world. Among his accomplishments was the development in 1960 of a theory for turning a sphere, such as a basketball, inside out without cutting or creasing it.

He was also well known, at least in some circles, as a political activist. A penchant for social causes had been with him for many years. In his late teens he took part in a protest against a ban on the teaching of evolution at his high school. During his undergraduate days at the University of Michigan, the authorities placed him on probation because of his refusal to cooperate in the investigation of a private dinner for a speaker who had been barred from the campus.

None of these extracurricular activities impeded his professional progress. He got his doctorate at Michigan in 1956. He then spent 2 years as an instructor at the University of Chicago, followed by 2 years on the staff of the Institute for Advanced Study at Princeton. In 1960 he began his first tour of duty at Berkeley as an associate professor. A year later his appointment to Columbia University is said to have made him the youngest full professor on the faculty of any major American institution of higher education at that time. In 1964 he returned to Berkeley in the same capacity. There he threw himself into the Free Speech Movement and acted as faculty adviser for the Fair Play for

Cuba Committee. Later he cofounded the Vietnam Day Committee and helped plan and lead its most publicized endeavor, the attempt in 1965 to stop the trains bearing soldiers to California ports for embarkation to Vietnam.

Early in 1965 a grant from the National Science Foundation, eventually amounting to $58,900, enabled a group of researchers at Berkeley to begin an investigation in higher mathematics under Smale's guidance. The young topologist kept this position, that of principal investigator, when in 1966 a second NSF award, this one for $91,500, allowed Berkeley to continue the project for 2 more years on a somewhat expanded basis.

It was under this second grant—listed in the NSF files as GP-5798—that the Smale affair erupted with much of the controversy revolving around two of the award's provisions.

One of them had to do with Smale's personal stipend from the grant funds. This was to take the form of payment for 2 months of research in the summer of 1966 and for another 2 months during the following summer. The total, $11,112, was the equivalent of his university salary for such periods with the amount payable for each month set at $2,778, one-ninth of Smale's school-year compensation.

These terms conformed to Foundation policy concerning the extent to which a college or university can reimburse itself for salaries to staff members doing grant-financed work. Formulated in 1952, the policy forbids the use of NSF funds "to increase the monthly rate of compensation of staff members of institutions . . . engaged on research supported by the Foundation." If the time required for the research makes it impossible for a staff member to perform some of his regular academic chores, the institution as a rule can recover an appropriate percentage of his pay out of grant funds. Similar conditions cover the summer recess. During this period, however, NSF money may be applied to salary for no more than 2 of the 3 summer months.*

Another provision of GP-5798, destined to figure large in subsequent news stories, earmarked $1,000 of the grant funds to assist one of the project researchers to attend a 10-day session of the International Congress of Mathematicians, scheduled to convene in Moscow in August 1966. The figure set in this clause reflected long-established NSF policy, limiting the sum allowable for travel to a general conference in a foreign country to the roundtrip jet economy air fare between the researcher's institution and the site of the conference.

---

* Over the years NSF policy on summer salary has undergone several changes.

That the University of California would make the travel allotment in GP-5798 available to Smale was a foregone conclusion. For one thing, the managers of ICM had invited the Berkeley topologist to read one of the 12 major papers to be heard at the Moscow meeting. For another, the agenda of the Congress included the presentation to Smale of the J.C. Fields Medal for his theorem concerning regular maps of the sphere. This honor, awarded infrequently by the International Mathematical Union, has been called the "Nobel Prize" of mathematics.

A quadrennial event, ICM is the only international meeting of its kind. In the hopes of encouraging a large representation from this country, NSF had made a travel fund available at the National Academy of Sciences by means of two block grants, $42,480 in all, to that organization.

In turn, the Academy invited applications and announced that successful candidates would receive $400 each. As the number of applicants was large—635—the Academy required all candidates who had other sources of travel funds to list them. When in October 1965 Smale submitted an application, the second grant for his Berkeley project (GP-5798) was under consideration at NSF but had not yet been issued. In accordance with the rules, Smale stated that he had applied for a travel allowance under the forthcoming grant. He also agreed to apprise the Academy if his application were honored.

In November the members of the Academy's travel committee wrote him that he was one of 127 successful applicants and repeated their wish to be notified should other such funds become available to him. On March 1, 1966, they sent him a check for $400. By this time GP-5798 had taken effect, but Smale neglected to tell the Academy that he now had access to the $1,000 travel fund provided by the new NSF grant. A few weeks later he requested sufficient advance against it to cover his air fare, $418.30, as far as Paris. Simultaneously, he indicated to the accounting office at Berkeley that he would request additional travel money from the same source at a later date.

In late May he left for France with his wife and children. During a short stay in Paris he learned that some fellow mathematicians there were drafting a declaration against American participation in the Vietnam War. He later described his role in this activity as "a minor one." According to another source it consisted of taking part in a "teach-in" on the subject. He spent most of June and July in Geneva, doing mathematical research and attending seminars. In August, having installed his family in Greece, he headed for the big conference in Moscow.[1]

What do we have so far? Access to a little more travel money than either the Academy or the Foundation had intended Smale to enjoy.

What are we about to have? Headlines in the press, outrage in quarters of the American mathematics community, inquiries from Congress, and headaches for NSF Director Haworth and his staff.

Once under way, the Smale affair developed swiftly. On August 5 the *San Francisco Examiner* published a little story under a large headline saying that the University of California topologist had fled to Europe to avoid service of a subpoena that the House Un-American Activities Committee (HUAC) had issued in connection with its hearings into demonstrations against the Vietnam War.

On the following day the *Examiner* took it all back. Prodded by phone calls from the mathematics department at Berkeley, the newspaper apologized for the "inferences" in its first story. Smale, it revealed, had planned his trip abroad long before HUAC had acted. The subpoena had not been served simply because at the time of its issuance the young topologist was already out of the country.

By the 12th, the American academic world had mobilized. To Congress went a protest signed by 260 professors, many of them mathematicians. HUAC's release of the names of persons for whom summons had been issued, the professors asserted, violated "the common standards of decency and fair play." It also violated HUAC's own rule stating that no member of "the Committee or staff shall make public the name of any witness subpoenaed before the Committee . . . prior to the date of his appearance." The professors accused the House group of subjecting Smale "to damaging smears and innuendoes."[2]

Following the stories in the *San Francisco Examiner* in early July, Smale's name disappeared from the headlines. Then came the opening in Moscow of the International Congress of Mathematicians on August 16, followed shortly by a couple of articles about Smale in the *New York Times.*[3]

On the first day of the Congress, the Berkeley topologist accepted his medal and the $1,500 that accompanies it at a ceremony in the Kremlin. After that the conference moved to the 35-story skyscraper building of the University of Moscow in the Lenin Hills overlooking the city. Of the 5,000 delegates on hand from 58 countries, almost 500 were from the United States.

On the 17th, according to the *Times,* Smale was "busy trying to organize a . . . demonstration against the Vietnam war as an added feature of the International Congress of Mathematicians." Petitions subsequently circulated by a small group of his supporters urged ICM to go on record in a denunciation of both American foreign policy and of what they described as HUAC's attempt to spread the impression that Smale had skipped the country to escape a summons.

In Washington, the National Academy of Sciences acted. On the 24th its section on the U.S.S.R. and Eastern Europe dispatched a memo to the Moscow conference, addressed to all of this country's "Official Delegates and Alternates." Attached was a copy of the latest story from the *Times*. "Officials of the Department of State," the memo read, "have asked the Academy for information about the activities reported in this article. . . . We would appreciate any comments you may be able to make to us about the political subjects raised in the context of the Congress."

By this time the petitions distributed at the ICM by Smale's friends had fallen before the refusal of an overwhelming majority of the delegates to mix politics and science. Unable to move the Congress to action, the Berkeley mathematician took his cause to the world. At 10 o'clock on the morning of August 26 he held a press conference on the front steps of the University of Moscow.

Smale's own account of this event traces its beginnings to a few days earlier. "In the middle of the conference," he later told *Science* magazine,[4] "I and two others were invited to dinner by some North Vietnamese mathematicians. They asked me to give an interview to the North Vietnamese press. I was hesitant because I was afraid of the use they might make of it. So I insisted that the American press be present. Both American and Russian newspapermen were then invited."

On the night before the press conference all this changed. Without explanation the Moscow correspondent of the North Vietnamese Press Agency, Hoang Thinh, informed Smale that he would not be present. Instead, he was submitting written questions.

Next morning Smale opened his conference with some remarks he had written in ink on two pieces of lined paper. As he read, delegates to the Congress streamed by, en route to the closing-day sessions in the towering building behind him. Smale condemned American military intervention in Vietnam as "horrible." Then, in what he later described as an effort to be "fair and balanced," the professor reminded his auditors that only 10 years before the Soviet army had "brutally intervened" in the Hungarian rebellion. Next he turned his attention to HUAC, asserting that there was "a real danger of new McCarthyism in America as the result of the Committee's efforts to suppress "the right to protest." He followed this with another "plague-on-both-your-houses" addendum. In talks with Russian intellectuals at the Congress he had discovered that many were "discontented" about the recent trial and imprisonment of two Moscow writers accused of having smuggled anti-Soviet literature abroad. "The lack of means to express this discontent," Smale said, "is a sad state of affairs."

His prepared statement finished, he took up the written questions from the North Vietnamese agency. Suddenly, according to the *New York Times*, a middle-aged woman appeared. Touching Smale on the shoulder, she said that Vladimir G. Karmanov, secretary-general of the Congress, wanted to see him at once for an "urgent" discussion.

For a brief period Smale remained where he was, answering oral questions. Then he entered the building and went to Karmanov's office. He was closeted there for about 15 minutes. When he came out, he had company: Karmanov, an interpreter described by the *Times* as a "rosy-cheeked woman," and "two burley men." Smale was carrying a gift, presented to him in the office. It was a book about the Kremlin, well-illustrated.

As the professor and his escort moved quickly down the front steps in the direction of a waiting car, a gray Volga, reporters' attempts to question him further were shunted aside.

"This is a mathematics conference," the rosy-cheeked interpreter told them with an ingratiating smile. "Mathematics must come first. You can talk with the professor later if you wish."

One eyewitness got the impression that the professor was about to say something nonmathematical, only to think better of it as his escorts quickly ushered him into the Volga and got in with him. As Smale later remembered the ride, there was a little talk about "museums and interesting places" as the car headed for the center of the city at high speed. He had no idea where he was being taken until the Volga pulled up outside of Novosti, a Soviet news and publicity agency, and he was hustled inside.

There he remained about an hour. His hosts asked him for a copy of his prepared statement. He gave them one. It was handwritten, like the original. They required him to repeat the answers he had made to the North Vietnamese correspondent. He did so. There was more talk about museums—which ones had Smale visited while in the city?—before he was finally returned to the university.

"It was all rather confusing," he ruminated that evening. "It seemed to be a rather rude attempt to keep me from talking with Western correspondents."

During the next few weeks the American press came close to converting Smale's name into a household word. Provocative headlines sprang up the country over: "Moscow Silences a Critical American"—"Subsidized Slander"—"Says U.S. Pays Tab/As Prof Backs Foe." Some newspaper accounts had Smale receiving as much as $13,000 from the Foundation in travel expenses alone. Others, also inaccurate, pictured him as spending the summer delivering speeches against the Vietnam War.

Disturbed by these reports, citizens got in touch with their Senators and Congressmen, who in turn requested information from NSF. Many of the press clippings accompanying constituents' protests quoted from a news release issued by Representative Richard L. Roudebush. The Indiana Republican demanded that the Foundation cancel the remainder of the Berkeley grant insofar as it applied to Smale. He didn't believe "American taxpapers should be saddled with the expense of sending a professor on a 3-month European junket to issue statements in Moscow running down the policies of the United States Government."

At the National Science Foundation the staff was busy. Too busy, some members would say later, contending that Director Haworth "overreacted" to the Smale Affair.

Haworth insists that he was not "particularly agitated" when the staff first called the matter to his attention. "Certainly not by those remarks of Smale at his press conference in Moscow," he says. "I remember thinking, 'this is just one of these things.' On the other hand I realized it could boom into something. Naturally I encouraged the staff to continue looking into it."

The Director says that he didn't start "really worrying" until he learned that a reporter, catching up with Smale in Europe, had asked about the HUAC subpoena and that Smale had answered that the summons hadn't been served because when it came out he was "vacationing" in Greece.[5]

Actually most newspaper stories quoted Smale as saying he was "traveling" in Greece. But whether the word was "traveling" or "vacationing," it had a mournful sound at the Foundation. Under the terms of GP-5798, NSF had reimbursed the University for Smale's salary for July. It was supposed to do the same for August. But what was the professor doing in Europe during those 2 months? Was he researching or junketing? At the end of August that question dominated 11 phone calls and almost as many letters from NSF to Berkeley.

The University authorities had no clear idea of what Smale had been doing. They informed the Foundation that the information it sought could not be supplied until the Professor got back to this country and had submitted a report on his travels. Pending these occurrences, the Foundation, on September 1, informed Berkeley that NSF could not reimburse the university for the professor's August salary. At this point Smale and his family were heading homeward, moving by car from Greece by way of Istanbul and Paris to Le Havre where on September 9 they boarded the S.S. *France* for America.

On the 14th the Smale Affair was a subject of discussion, for the second time, on the floor of the House of Representatives, and at 10:30

that evening the S.S. *France* docked at New York. From there Smale and his family motored to New Jersey, the professor having previously arranged with his university to spend the first quarter of the school year working at the Institute for Advanced Study in Princeton.

From here, on the 16th, he submitted a review of his summer's mathematical activities to Vice Chancellor Robert E. Connick at Berkeley. Smale wrote that he considered himself as having "spent full-time this summer on mathematical activity . . . whether measured from time spent . . . or on results produced." He mentioned the universities where he had worked, and a 6-day conference he had attended at Bonn, Germany, and pointed out that he had also done mathematics "in campgrounds, hotel rooms" and on the SS. *France* where he "discussed problems with top mathematicians and worked . . . in the lounge. . . ." He reminded Connick that his "best-known work was done on the beaches of Rio de Janeiro" in 1960, and added that he resented "your stopping my NSF support money for superficial technicalities. The reason goes back to my being issued a subpoena by the House Un-American Activities Committee and the subsequent Congressional and newspaper attacks on me."

Connick transmitted this information to the Foundation by telephone. That organization, meanwhile, had obtained a security clearance on Smale from the Federal Bureau of Investigation[°] and on the 20th it made clear that it had no intention of withdrawing support from Smale and that the University of California would be allowed to pay his August salary out of NSF funds. In a statement, later drafted for circulation to public and Congress, Director Haworth, described this decision as in line with NSF policy as formulated in 1954. The gist of this policy, first published in the Foundation's 6th Annual Report in 1956, was that although NSF would not knowingly support an avowed Communist or anyone established as being so by a proper judicial or governmental authority, it would appraise "the merit of a proposal for unclassified research submitted by or on behalf of a scientist" solely in the light of "his experience, competence, and integrity." With respect to the salary, NSF had satisfied itself through the University that, including parts of May and June, Smale had indeed worked the 2 months for which he was being paid.

Having affirmed its continuation of support for Smale's research, the Foundation almost at once took a step aimed at preventing future embarrassments of the sort he had created. On the 26th an NSF interoffice circular announced that "Henceforth funds for travel to international conferences held outside the United States will not be provided through research grants. Requests that funds budgeted for other purposes be diverted to [such] travel . . . will not be approved." The already existing mechanism of travel grants should be used instead. Excepted from this ruling were meetings of United States scientific societies in Canada or Mexico.[7]

As for Smale's travel expenses: in an informal accounting to the Berkeley authorities, the Professor acknowledged getting $400 for such purposes from the National Academy of Sciences and $1,000 under the NSF grant, GP-5798. He estimated his actual expenditures as at least $1,599. The decision of the Foundation was that Berkeley could charge only $619 of these costs to the travel grant allotment in GP-5798—the difference between the $400 Smale had received from the Academy and $1,019, the price of a roundtrip jet economy ticket between San Francisco and Moscow.

The agency's determination to continue its support of Smale's work loosed another flurry of newspaper headlines. This one was shortlived, but as public interest in the case subsided (temporarily, as it turned out), staff members at NSF found themselves coping with a related problem, thanks to the epistolic vigor of Professor Leon A. Henkin, chairman of the mathematics department at Berkeley.

As a delegate to the International Congress of Mathematicians, Dr. Henkin had witnessed the celebrated press conference on the steps of the University of Moscow. Later he had defended Smale's right to speak his mind.

Like Smale, Henkin was doing research under an NSF grant. On his return from the Moscow conference he had submitted a report of his expenses only to be told, as had Smale, that NSF would refund nothing beyond the price of a roundtrip tourist-class air ticket.

In the closing months of the year the Berkeley department head was writing frequently to NSF officials. He accused the agency of acting "unfairly by seeking to enforce a policy in the cases of Professor Smale and me which was not adequately brought to . . . our attentions . . . prior to our commitment of expenses" for the trip to Moscow.

One of Henkin's arguments in defense of his charge was that the only published statement of Foundation policy on foreign travel was open to more than one interpretation. Contained in a June 1963 brochure, that statement said that "Allowance for air travel will not normally exceed round trip jet economy air accommodations."

What did those words signify? Henkin had put that question to Vice Chancellor Connick of Berkeley and to August Manza, head of the University's Contracts and Grants Office. Both had agreed that the words had only one clear meaning, namely that NSF would not finance first-class air travel. None of the Berkeley officials saw anything in the Foundation's policy statement to rule out payment by the agency of a per diem to help defray other travel costs, such as hotel bills and meals.

Another of Henkin's arguments dealt with a May 25, 1966, communication from Dr. William H. Pell of the Foundation to W. Hesson of the Berkeley Research Office. In this letter Pell quoted an NSF circular of 1964, stating that "under ordinary circumstances the use of grant funds for per diem for international travel should not be allowed."

Explicit enough? Yes, said Henkin, but neither he nor Smale had been aware of Pell's letter when they made their arrangements for the journey to Moscow. In view of this fact Henkin concluded that where he and Smale were concerned NSF's interpretation of its policy on foreign travel represented an effort to punish the two of them for their political views.

The argument by mail had dragged on for months when on April 27, 1967, Director Haworth undertook to resolve it personally. Writing more in sorrow than in anger, Haworth deplored what seemed to him to be Henkin's excessively technical approach to the issue. The Director looked on the rapport between NSF and the American scientific community—a valuable relationship in his opinion—as deriving its strength not from narrow adherence to bureaucratic protocol but from the exercise of good faith on both sides. "First," he wrote Henkin,

> let me say that those who interpret any of the Foundation's actions as punitive measures are badly mistaken in their beliefs. . . .
>
> In the case of the Foundation's interpretation of allowable costs connected with . . . travel to the ICM the same ruling was applied to everyone, to the best of my knowledge. . . .
>
> With these remarks as background you may appreciate the value which Foundation officials attach to retaining flexibility in the conduct of their programs. It has been my hope that misinterpretations of the Foundation's position in a variety of matters, when they arise, can be met simply and with confidence in the good intent of the motives of both parties involved. An inability to operate in this manner will lead to rigid administrative practices which can only reduce the effectiveness of our contribution to U.S. science as a whole.[8]

Haworth's thoughtful letter ended the fuss over the travel issue. It did not end the Smale Affair. With the coming of warm weather that cause celebre bounced back into the headlines following a swift sequence of events.

On May 29 the University of California, Berkeley, asked the Foundation for a third grant—to continue the Smale mathematics project for another 2 years, with a much larger number of workers and a greater variety of work.

No bells pealed at NSF when this application arrived, calling as it did for considerably increased responsibilities for Smale. The Foundation's response was in the nature of a counter offer, first conveyed to the University informally, latter by official letter. The new application contemplated a larger number of researchers and a greater variety of work than previous Smale grants. NSF's suggestion was that the requested grant be split in two. Under this arrangement Smale would administer those aspects of the study that he and his close collaborators were planning to pursue. One or more Berkeley mathematicians would take charge of the remaining elements. If this proposition were acceptable to the University, the Foundation stood ready to issue two or more grants.*

Running through the stormy public reaction to this counter offer to Berkeley was the nagging question of motive. Were the Foundation leaders trying to punish Smale for his political outburst in Russia, or did their demand that the new grant be split reflect an honest dissatisfaction with the Berkeley topologist's managerial capabilities?

Answering those questions today, Haworth declares that "political considerations did not enter into our decision at all. We wanted to make it possible for Smale to continue his work. At the same time we wanted to limit his supervisory duties as much as possible for the reason that his conduct had convinced us that when it came to running things he was inept. You'll remember, for example, that when he obtained a travel allowance from the National Academy of Sciences, he agreed to notify them if further travel money became available to him and didn't do so. You'll also recall that he returned from Europe on a French ship, knowing full well that people traveling on Government funds are required to use American carriers whenever available. There were times when he paid little or no attention to correspondence. Obviously he was a man who didn't care to play by the rules. In our view that added up to a lousy administrator."[9]

As the Smale Affair surged into the headlines for a second time, the Foundation found itself running the gauntlet, with the forces of the anti-Communist right virtuously arrayed on one side and those of the left similarly alined on the other.

Newspapers quoted Representative Roudebush as asserting that if NSF extended additional money to Smale, the Indiana Republican would ask Congress to veto the action. In a widely published article,

---

* As eventually worked out two grants, each for $87,500, were issued.

Washington correspondent Ralph de Toledano reviewed Smale's career as an activist and marveled at Haworth's "callousness" in remaining "unperturbed" in the face of such a background. If the far from callous NSF head were not already deeply upset, he became so on reading a story in the *San Francisco Examiner*[19] quoting someone at the Foundation—erroneously, according to Haworth—as saying that the new Berkeley application was being subjected to "political considerations."

Commentators on the other side of the ideological gulf saw in the development a threat to academic freedom. Writing in the *Washington Evening Star*, William Hines wondered if a public agency could "properly take sanctions (if that is what NSF is doing) against a critic of government policies who has the poor judgment or poor taste to voice his criticisms abroad? . . . if so, can an agency go one step further and take sanctions against all political opponents, whether or not they speak out?"

Quoting one of Congressman Roudebush's aides as saying Smale was "as pink as they come," the liberal weekly, the *Nation*, observed that "a pink topologist furnishes a prime issue for a conservative Congressman who must run for election every 2 years." The magazine discerned in NSF's handling of the new request from Berkeley ample reason for American scientists to "rage and burn."

Some did. At the University of Pennsylvania 50 faculty members, in and out of the mathematics department, affixed their signatures to a statement saying that "Unless there is an acceptable explanation of the rejection of Stephen Smale's contract application the undersigned cannot accept for personal use any funds from the National Science Foundation"—an action that struck one American journalist as suggesting the existence among the educators of "a kamikaze streak . . . heretofore . . . wholly absent from academe's dealings with the Federal Government."

One university of Pennsylvania scientist, a professor of geophysics, did not go along with his associates. To an NSF official he described Smale's political views as "not too different from my own." He quoted his colleagues in mathematics as saying that Smale's "contribution to topology is very great," adding that "I believe it, but I am prepared to raise the question whether the damage he has done to the image of American science, to intra-Soviet science management, and to Soviet-American relations may not be even greater. His personal irresponsibility clearly disqualifies him for a position in the administration of any large project."

Smale himself was active. From the Battelle Memorial Institute's new research center in Seattle, where he was spending part of the summer, he endeavored to rally his fellow mathematicians. He told reporters that in attempting to split the grant the National Science Foundation had "dishonored itself." He pictured the agency as "walking a tight-rope between Congress and the scientists, not wanting to offend either one."

NSF's formal request that the grant be split, mailed on the last day of August, was addressed to Sanford S. Elberg, Dean of the Graduate Division at Berkeley. Elberg sent it to Smale who promptly made its contents available to Daniel S. Greenberg, an experienced Washington reporter with a Puritanical intolerance for human weakness, a sharp mind and a gifted pen. Now publisher of the newsletter, *Science and Government*, Greenberg in the summer of 1966 was manning the news desk of *Science* magazine.

Greenberg got in touch with Dr. Philip Handler, head of the Department of Biochemistry at Duke University and Chairman of the National Science Board. He told Handler that a sentence in NSF's communication to Elberg had convinced Smale that the Foundation was trying to reduce him to the status of "a mere graduate fellow." Handler, in a subsequent memo, expressed the belief that in the course of an hour-long interview he had disabused the *Science* reporter of this misapprehension.

On September 10, a Sunday, Greenberg called Director Haworth at his home. Not wishing to be publicly critical of Smale or the University, Haworth refused to discuss the case on the record. At the reporter's urging he did explain, off the record, why the Foundation people were dissatisfied with Smale's administrative performance.[11]

"Naturally," Haworth says today, "I was pretty annoyed when in the next issue of *Science* Greenberg repeated everything I'd told him. At that point I'm convinced the Smale case would have just faded from view if Greenberg hadn't decided to spend the next few weeks hammering away at it in his magazine. He kept stirring it up, looking for a crusade, I guess. Smale was writing the Foundation frequently, and I noticed that just about every time we got a letter from him its contents were in the next issue of *Science*. Obviously, he was sending copies to Greenberg before sending the originals to us."

At the University, meanwhile, the authorities were mulling over NSF's suggestion that the long controversy be resolved by splitting the grant in two. On the twelfth a delegation from the Department of Mathematics discussed the matter with Berkeley Chancellor Roger W. Heyns.

By this time Haworth had been informed that Heyns and other top administrators at Berkeley favored NSF's proposal but that the idea was still encountering a "buzz-saw" in the mathematics department.[12] Not a prolonged buzz, apparently, for toward the end of the month it was clear that the University intended to go along with NSF's suggested arrangement.

In one of his last articles on the subject Daniel Greenberg retraced the Smale Affair from its beginnings. Previously he had described NSF's decision to split the grant as "bureaucratic weasling." He now concluded that the decision was an "honorable" one and that the agency had acted from the first in good faith. His only remaining charge was that NSF's handling of the problem was marked by political naivete, including "an appalling incapacity to distinguish the gnats from the tigers in the U.S. Congress." Greenberg's reference was to what he assumed to be the Foundation's exaggerated distress at the attacks by Congressman Roudebush. "Thus, last year," the reporter wrote in the September 27 issue of *Science*,

> when Roudebush was thundering imprecations at NSF, one of the Foundation's . . . statesmen woefully said, "We're in for it now. Roudebush is going to hold an investigation."
>
> When this mourner was asked if he had "talked to Miller?" he responded, "Who's Miller?" Then it was explained to him that George P. Miller (D-Calif.) is chairman of the House Science and Astronautics Committee of which Roudebush is a minority member; and that it was extremely unlikely that Roudebush would investigate anything without the approval of the chairman. . . ."

To Director Haworth these words were the unkindest cut of all. "I can't imagine anyone in the Foundation not knowing who Chairman Miller was," he says today. "Greenberg must have talked to the janitor. Not that there's anything wrong with janitors, you understand. When the Smale case began, one of the first things we did was to discuss it with Miller and with [Representative Emilio Q.] Daddario [of Connecticut], Chairman of the Subcommittee having cognizance over the Foundation. As for Roudebush, we were used to him. He'd been criticizing us for years."

On November 17 NSF issued the two grants, and the Smale Affair vanished at once and for keeps from the press—a fitting fate, it would seem, for a tempest in a samovar.

Chapter 10. Tempest in a Samovar

[1] Except as otherwise attributed, the information in this chapter is derived from a collection at NSF of press clippings, extracts from the Congressional Record, and copies of letters and other documents, assembled in two loose-leaf notebooks and labeled "Smale."

[2] *Cong. Rec.*, 90th Cong., 1st Sess., p. A4353.

[3] Aug. 17 and Aug. 21, 1966.

[4] Oct. 7, 1966.

[5] Personal interview.

[6] LJH to Joseph A. Califano, Jr., Special Assistant to the President, Sept. 8, 1967, Chron File.

[7] Assistant Director/Research, Circular No. 31, Sept. 26, 1966.

[8] LJH to Henkin, April 27, 1967 (copy of Henkin to LJH, Mar. 15, 1967, attached), Chron File.

[9] Interview.

[10] July 6, 1967.

[11] Handler, memo (NSB-67-191) to members of Board ("Subject: *Article in Science Magazine*"), and in Chron File: Haworth, memo to NSB, Oct. 6, 1967.

[12] LJH to Elberg, Oct. 12, 1967, Chron File.

Chapter 11

Project Mohole and JOIDES

Another newsmaking episode of the 1960's followed the Foundation's decision at a somewhat earlier date to support a remarkable venture into big science known as Project Mohole.

Few NSF activities have attracted more public attention. None has been the butt of harsher criticism. Investigative journalists dubbed the project a "scandal,"[1] a "fiasco,"[2] a case of the "fragile flower" of pure research "wilted by bureaucratic contamination."[3] Magazines ran accounts of its progress under put-down headlines, such as "Mohole may Wind up No Hole?"[4] and "Mohole Regress Report."[5] As the mishaps and the confusions multiplied a controversy over goals and procedures had the effect of driving the first serious rift between the Foundation and elements of the American scientific community.

With these doleful matters as background, and bearing in mind that time puts old events in new perspective, we can review the highlights of the only NSF activity to be terminated by an act of Congress.

Project Mohole (so named for reasons soon to be obvious) was a 10-year effort by American geologists, geophysicists, geochemists, oceanographers, and other scientists to take a look at the deep interior of the earth. It had been known for many years that the outer skin of the earth—the crust—ranges in thickness from as much as 50 miles on land to as little as three miles beneath the ocean. Underlying the crust and wrapping the earth's core is a far larger segment designated as the mantle. The juncture between crust and mantle is called the Mohorovicic discontinuity or Moho after the Croatian-born geologist, Andrija Mohorovicic, who first recognized its existence in 1909.

The goal of the fathers of Project Mohole was to drill through the relatively thin crust at the bottom of the sea, pierce the Moho and recover cores of the mantle rock. They believed that an examination of these samples would tell them a good deal about the origin, evolution, and age of the globe; its radioactivity, heat balance, and the variations in its magnetic field, even perhaps about the mechanics involved in the formation of mineral deposits and fossil fuels.* By the time the project came along in the late 1950's an itch to punch deep holes for the purpose of seeking answers to these and other geological questions had been epidemic along earth scientists for well over half a century. Indeed a fair number of such holes had been drilled, none, however, even close to Moho depth.[6]

The first direct suggestion that geoscientists dig deep enough to sample the rock of the mantle seems to have come from Frank B. Estabrook of the Basic Research Branch of the U.S. Army. In a letter in the October 12, 1956 issue of *Science*, Estabrook argued that inspection of the material below the discontinuity would shed light on many geophysical mysteries. His idea invoked considerable interest but, as a participant in project Mohole later observed, ideas in themselves "have no momentum." To produce anything they must be joined in wedlock with "organization and determination."[7]

The requisite marriage began to take form on March 23, 1957, during the closing hours of a 2-day meeting at the National Science Foundation of its Advisory Panel for Earth Sciences. Present, among others, were Harry H. Hess of Princeton University and Walter H. Munk of the Scripps Institute of Oceanography of the University of California, San Diego, both towering figures along the geophysical-oceanographic axis.

Several first-hand accounts of what happened are available. One is from William E. Benson, then NSF's program director for the earth sciences, later head of that section of its Division of Environmental Sciences.

"We'd reviewed 65 proposals, applications for grants," Dr. Benson recalls. "Many were good, but there was an unease amongst us, a feeling, incidentally, that had been permeating the whole field for some time. It was the feeling that the earth sciences were stuck on a plateau. The time had come for us to start climbing, to get behind something big and exciting—a project that would either help confirm old notions about the earth or generate new ones. But what should it be, everybody wondered; and the next thing you know Munk was saying in effect, 'Let's go to the Moho,' and Hess was suggesting that we put the question of whether it could be done up to AMSOC."[8]

---

* Describing the scientific objectives of Project Mohole in a long letter to Charles L. Schultze, then (1965) Director of the Bureau of the Budget, Dr. Haworth pointed out that "We are at the beginning of planetary exploration and it is important that the earth also be studied as a body in space. . . . Within the earth's mantle lie the sources of stress that cause the great tectonic features that confront the geologist. These all must somehow be explained: volcanism, mountain building, earthquakes, continents and oceans. The geologist has always had to rely upon what he finds in the rocks in order to interpret the history of the earth and to infer the processes that have operated to produce the features at the earth's surface. . . . Our theories about the earth have been invented largely from what we know of less than 1 percent of its volume and 30 percent of its surface. We have never had rocks from the deep crust or mantle, and until quite recently, we have not even had rocks from the top of the oceanic crust or the deep sea basins. As in all science, we must now check theory with direct evidence if we are to progress." (Aug. 21, 1965, Chron File.)

AMSOC? The American Miscellaneous Society, a group of earth scientists wont to foregather from time to time and from place to place to discuss wild but in some instances potentially valid ideas too nebulous for presentation at standard scientific conclaves. AMSOC had no officers, no by-laws, no dues. It eschewed committees. All it had was a handsome letterhead and five divisions labeled Etceterology, Phenomenology, Calamitology, Generalogy, and Triviology. If a member found these categories too confining, he was privileged to found his own division. Or he could avail himself of AMSOC's affiliations with the Committee for Cooperating with Visitors from Outer Space and the Society for Informing Animals of their Taxonomic Positions.

Obviously AMSOC was the logical group to look into the possibilities of a *Voyage au Centre de la Terre.* The members rose to the occasion at once. At a meeting in California on April 20, 1957, they abandoned their carefree ways to the extent of establishing their first formal division, the Committee on Deep Drilling. A week later, in Washington, the six members of this body met for an organizing session, named geophysicist Gordon Lill of the Office of Naval Research as chairman, and asked the National Science Foundation for funds to carry out a feasibility study.

The NSF leaders said no. They could not grant public funds to a committee whose parent body was little more than a peregrinating scientific seminar. They let it be understood, however, that they might reconsider if Lill and his colleagues could see their way to affiliating with a legitimate organization.

No problem about that. AMSOC-member Hess was chairman of the Division of Earth Sciences of the National Academy of Sciences-National Research Council. Before you could say American Miscellaneous Society, the term AMSOC has become not only the acronym of an informal organization but the name of one of the hundreds of formal committees operating under the aegis of the NAS-NRC.[9]

Satisfied with this arrangement the National Science Foundation began issuing grants: "up to $75,000" for feasibility studies, $80,500 to NAS-NRC to enable its new AMSOC Committee to hire a technical staff.[10]

To head the staff Hess handpicked Willard N. Bascom, a 44-year-old, Manhattan-born research engineer. Brilliant and articulate, Bascom had worked as mining engineer in the West, directed a study of beaches, waves, and shorelines for the University of California, Berkeley; lectured on oceanography at the Navy's postgraduate school; and helped monitor the effects of nuclear bomb tests on the American proving grounds in the Pacific. In 1954 he joined the NAS-NRC staff. He was serving as executive secretary for two other Academy committees when in July 1958 Hess tagged him for the AMSOC group where Bascom's drive and inventiveness made him for the next 2 years the prime mover in the development of Project Mohole.[11]

To direct the feasibility investigation and collate findings, the AMSOC Committee enlarged its membership and created study groups. One, guided by Hess, looked for appropriate drilling sites in the ocean. Another, chaired by veteran petroleum geologist Walter B. Heroy, Sr. and known as the Panel on Drilling Techniques, began studies looking forward to the development of effective equipment.

Throughout 1959 and well into the next year staff and panels grappled with a miscellany of questions.

Was the Mohorovicic discontinuity within reach of modern drilling machinery? Their eventual answer—yes—rested on the assumption that by coring through a thin portion of the ocean bottom they could reach the discontinuity at somewhere between 31,000 and 35,000 feet below the surface of the water. This figure did not strike them as too much beyond the 25,340 feet of the deepest hole ever drilled as of that time—the I-EE University Well sunk by Phillips Petroleum in West Texas.

But the I-EE University Well had been drilled on land. Could the same operation be performed at sea? Originally the progenitors of Project Mohole had taken it for granted that the work would have to be done from an ocean island. This was a troubling prospect as it might call for deeper digging than even the best modern equipment could manage. Inquiry, however, revealed the development by the petroleum industry of ships and barges capable of drilling in water.

Could one of these vessels be adapted to Mohole purposes? As none had the capacity for reaching to the discontinuity, the question was really: On which of them would the project scientists be most likely to acquire the experience they would need to devise a vessel sophisticated enough to accomplish their objective? By mid-1959 the AMSOC people had concluded that their best bet by far was a onetime Navy freight barge that had been converted into a drilling craft and was now owned by the Global Marine Exploration Company of San Diego, Calif.

With a 98-foot derrick straddling a large hole in the center, this 260-foot, 3,000-ton craft derived its name, *CUSS I*, from the initials of the four oil producers—Continental, Union, Shell, and Superior—

who had sponsored its design as a drilling rig. *CUSS I* had functioned effectively in the relatively shallow waters along the continental shelf. Could it be modified for use in the open sea where success would hinge on the ability of the vessel to hold almost stationary in the presence of heavy waves, swift currents, and strong winds? Were it to stray more than 1,000 feet from center in deep water, or much less in shallow water, the long drill pipe would snap, aborting operations, jerking the craft, and endangering the crew. Worked out largely by Bascom, the solution for this problem was a dynamic positioning system that involved the use of radar and sonar buoys in conjunction with the powerful outboard motors of *CUSS I*.

In May 1960 the Foundation gave its blessing to an AMSOC Committee plan for carrying out the project in two stages. Under what later became known as Phase I, the Mohole people were to use *CUSS I* as modified to drill into the sea bottom in an effort to core the loose and coagulated upper layer of material known as sediment and penetrate the so-called basement rocks that mark the beginning of the lower layers of the oceanic crust.* If these drillings yielded sufficient engineering and scientific information, Phase II would follow. It would begin with the creation of a floating rig capable of piercing to the Mohorovicic discontinuity. It would end, if all went well, with the recovery of samples from the mantle. During the closing weeks of 1960 NSF authorized $1.25 million (later lifted to $1.8 million) to cover the costs of the first phase, and on New Year's day Bascom and his technical staff headed for San Diego to get it started.

As *CUSS I's* four outboard motors were for maneuvering purposes only, the barge had to be towed to the two sites that the AMSOC Committee had selected. For nine days beginning March 6, 1961, operations took place in 3,140 feet of water off La Jolla, Calif. At the March 16th meeting of the National Science Board Waterman was able to report that the drill had penetrated to 1,043 feet under the ocean floor. Five cores of sedimentary material had been extracted from depths ranging from 250 to 750 feet below the floor.

Shortly before midnight, March 23, the [illegible]S arrived at the second site, 44 miles east of Guadalupe Island a[illegible] 220 miles south of San Diego. Here the water was 11,672 feet deep—ten times deeper than that in which any oil drilling had been carried on effectively. During 4 days of operations the drill went to 601 feet below the ocean bottom. On Easter Sunday it penetrated 44 feet of basement rock (basalt) and brought up the first samples ever recovered from the second layer of the oceanic crust. The word throughout the geological-oceanographic world was that Phase I had been a resounding success.[12]

---

* What distinguishes the various layers of the oceanic crust is the velocity with which each of them transmits seismic waves, as measured by instruments set up on islands and atolls.

Let it be remembered that these halcyon beginning days of Project Mohole were coeval with the establishment of the National Aeronautics and Space Administration and the initial moves in America's effort to land astronauts on the moon. As the first stage of the deep drilling venture ended and attention shifted to the next one, the inevitable in-house joke made the rounds at the National Science Foundation.

"While with its man-on-the-moon program NASA is trying to get to heaven," it went, "with Project Mohole we are heading as fast as possible in the opposite direction."

How fast would soon become apparent, for it was during the organizational shifts effected between Phases I and II that the groundwork was laid for the difficulties about to afflict Project Mohole.

So far the enterprise had been managed by the National Academy of Sciences-National Research Council through the AMSOC Committee. Now that the experimental first phase was over and the more "heavily operational" second one ready to begin, the Academy wanted out of its administrative duties. So did the Committee.

NAS-NRC wanted out in deference to its statutory position as adviser to the Federal science establishment, a function that the Academy regarded as requiring it to refrain from operational involvement in activities on which its counsel might be solicited. AMSOC's reasons were of another nature. The members of the Committee received no compensation and could give the project only whatever time and attention their regular occupations allowed. As Chairman Lill wrote, "the administrative demands of continuing work at sea, and the deliberate pace of Committee activities do not mix well. . . ."

But with both Academy and Committee gone from the supervisory picture, how was Phase II to be run? Worked out at meetings in May and June 1961 the Committee's answer to that question took the form of what Lill described as "an excellent set of guidelines."

AMSOC's number one recommendation was that henceforth the National Science Foundation assume full responsibility for Project Mohole. As the law forbade the agency to engage in scientific activities, the Committee suggested that the Foundation procure a prime contractor to provide the necessary equipment and to conduct drilling operations. As for the AMSOC Committee's role in the project, its members proposed that from now on they serve simply as scientific advisers to the Foundation, "concerned with matters of *scientific policy, engineering review,* and *budget.*"

Some of the Committee's other proposals dealt with matters shortly to become the focus of an unsettling quarrel. It had long been an article of faith among the AMSOC people (one accepted by NSF) that the attempt to pierce the mantle would have to be preceded by shallower corings in the oceanic crust on the order of and deeper than those conducted off Guadalupe during Phase I. Various new techniques would have to be developed, including means to withdraw the drill string in order to replace the drill bit, then to locate and reenter the hole. But would a special ship be needed for this intermediate stage of the second phase or not? In other words, to set forth at once the major issue of the impending controversy: *Should Phase II be a two-stage, two-ship affair, or should it be a two-stage, one-ship affair?*[13]

On June 12 the members of the AMSOC Committee's drilling panel, headed by Heroy, convened in Garland, Tex., to consider the problem. Their recommendation was that the project people not bother with an "intermediate ship" but proceed immediately to the development of the "ultimate" or Mohole ship. In the opinion of the panel members, the goal of the project could be satisfactorily accomplished by using the ultimate ship both for whatever intermediate drilling the undertaking required and for going to the Moho.[14]

Unlike its panel, the AMSOC Committee proper, convening in Washington a day later, was unable to reach a conclusion. The sense of the Committee's June 13th meeting was that the evidence on hand was not sufficient to support a firm decision and that Heroy's panel and the AMSOC technical staff headed by Bascom should get together for further discussions of the problem.[15]

Such was the standing of this aspect of things when on the 16th the AMSOC Committee's proposed guidelines went to Academy President Detlev Bronk for transmission to NSF. Prefacing the list were two categorical statements. One was that the "major scientific objective" of the project would be "to drill to the earth's mantle." The other was that this attempt should follow a series of shallower drillings aimed at the acquisition of both scientific and engineering information for use in accomplishing the ultimate objective of the project. On the question of whether one or two ships should be used, the recommendations were yes-and-then-again-no. Two sections of the guidelines advocated the development of an intermediate ship. A third made this action "contingent upon the findings of the Drilling Techniques Panel, working jointly with the AMSOC staff and eventually with the Prime Contractor." To which the guideline-makers added,

> This group may very well make decisions which increase the cost of the intermediate drilling program. Specifically, they may decide that an intermediate ship is not needed and that work on the ultimate ship should start at once.

Another recommendation by the AMSOC Committee was also pregnant with future trouble. It was that the prime contractor be required to make Bascom and his aides (the Committee's technical staff) "the nucleus of Phase II" by incorporating them into the contractor's organization.[16]

In late June Bronk endorsed the AMSOC blueprint for Phase II, Waterman accepted it with a few reservations as to detail, and the Foundation embarked on the business of selecting a prime contractor. This process began in earnest on July 27 when 200 representatives of 85 organizations gathered at the NSF auditorium to hear oral briefings covering Mohole requirements and to receive copies of a formal "Invitation to Submit Proposals."[17] It ended 8 months later with the Foundation's decision to entrust the future of Project Mohole to an engineering-construction firm, Brown & Root, Inc. of Houston, Tex.—a decision that in some quarters was bitterly criticized.

The formal invitation distributed at the July 27th meeting stated that proposals would be judged primarily "on the basis of engineering approach, skills, experience, and your independent appraisals as to how you would manage the entire program." No cost estimates were requested. The Foundation reasoned that the project entailed so many unknowns that such estimates would be meaningful only after a prime contractor had been chosen and had had an opportunity to analyze pertinent "research, development, and scientific data" and prepare "an engineering plan."

By September 11, the final date for acceptance of bids, 12 proposals were on hand. Dominated by oil companies, the list included also a scattering of engineering firms and independent or university-connected research institutions. Most of the offerings were not from individual organizations but from two or more organizations banded together for the purpose of pooling skills and capacities.

Waterman turned over the task of weighing the proposals to six members of his staff, later spoken of as the Preliminary Evaluating Panel. Guided by a variety of criteria geared to a point system, the panel gave first place to a combination of organizations headed by Socony Mobil. Associated with the big oil producer in the venture were General Motors, Texas Instruments, and Standard Oil of California. The panel reported that the proposal submitted by the combine, a presentation on which Socony Mobil and its associates had spent $150,000, was "outstanding in every respect and in a class by itself."

The combine's rating was 936 points out of a possible 1,000. Second place—902 points—went to the so-called "Gas Company," a combination of Global Marine Exploration, Aerojet-General, and Shell Oil. After that came Zapata Off-Shore Company with 812 points, General Electric with 811, and Brown & Root with 801.[18]

The preliminary panelists suggested that NSF begin negotiations with Socony Mobil at once. But Waterman was not to be hurried. Instead he established another evaluating unit. To this one he assigned four senior NSF officials. Their instructions were to review the findings of the Preliminary Panel. Simultaneously the NSF Director began soliciting the advice of "experts outside of the Government."[19]

While NSF pushed on with its search for a prime contractor, changes took place within the AMSOC Committee. In July Chairman Lill resigned. No longer connected with the Office of Naval Research, Lill left the Committee on learning that his new employer, Lockheed Aircraft Corporation, was then giving thought to entering the competition for the Mohole contract. Other members stepped out for the same reason. As a result the committee underwent a significant change of composition, accentuated in the fall of 1961 by an enlargement of its membership to 19 in an effort to make the project's scientific advisors as representative as possible of the numerous disciplines involved in the Mohole effort.[20] Several were oceanographers interested primarily in the sedimentary and upper crustal layers.

Among the newcomers was Dr. Hollis D. Hedberg, Professor of Geology at Princeton and Vice President in charge of exploration for Gulf Oil Corporation. A man of boundless energy and strong opinions, Hedberg tackled his committee chores with such zeal that in December, Academy President Bronk persuaded him to take over the chairmanship from its acting incumbent.

Even before becoming chairman, Hedberg had concluded that in drafting the original blueprint for Phase II, the AMSOC Committee had erred in some respects. He thought, for one thing, that the authors of the original blueprint had taken too flexible a stand on the matter of an intermediate ship. Although advocating such a vessel, they had conceded the likelihood that economic considerations might rule it out, in which case "work on the ultimate ship should start at once." Hedberg's position was that an intermediate ship was a must.

In his opinion the authors of the original blueprint had put too much emphasis on the attempt to go to the Moho and too little emphasis on the intermediate drilling program. What the AMSOC people had said about this in their original recommendations to NSF was that the intermediate drilling should be limited to the acquisition of scientific and engineering data needed for accomplishment of the ultimate goal, the penetration to the mantle. They had expressed the hope that some day NSF would support a broader exploration of "the sediments and upper crustal layers of the ocean basin." But as the job of coring to the mantle would "entail work enough," they suggested that this more general effort be regarded as a *separate* enterprise. Hedberg, on the contrary, was convinced that the more general program should be an integral part of Project Mohole.

Soon after assuming the AMSOC chairmanship, the Princeton geologist set out to induce the other members of the Committee to support his views. Partly because these views were often out of harmony with those of NSF, but chiefly because of Hedberg's practice of taking his case to the American scientific community via the press, the stage was thus set for an open conflict between the AMSOC Committee and the National Science Foundation.

Waterman, meanwhile, was finding himself hard put to react in a properly noncommittal manner to inquiries from members of Congress interested in seeing specific organizations considered for the prime contract. On October 18, 1961, he received a phone call from the office of Republican Senator Gordon Allott of Colorado. Waterman's record of the conversation quoted the senator's legislative assistant as asking "whether I had talked" with a man connected with Socony Mobil, "and I told him that I did."[21] Later the Senator himself called to remind Waterman "of the interest of his friend from Socony Mobil who had stopped in to see me . . ."[22] Still later Allott called again to say that he had heard that the contract was already awarded and to get assurances from the Director that such was not the case.[23]

Another problem encountered during the selection process was the question of what was to become of the AMSOC Committee's technical staff. Some of the entities competing for the prime contract stood ready to go along with the Committee's plea that Bascom and his assistants be taken into their organizations. From Brown & Root, on the other hand, came indications of an aversion to employing the staff as a unit along with an expression of willingness to use some of its members on an individual basis. Impatient by nature, Bascom refused to wait and see what happened. One week before the choice of the Houston firm was announced, he and his aides formed their own company, Ocean Science and Engineering, Inc.[24] At about the same time B&R's executive vice president, George R. Brown,* visited Washington and invited Bascom for drinks to discuss how things should be worked out if the Houston engineering firm got the contract. One account of this tete-a-tete at a Washington bar is that Bascom seized the occasion to tell Brown, in the presence of one of the latter's business associates, that he hoped his company did not get the job, that "they were wrong for it." Brown's reaction to this attack on his firm was to walk off.

---

* Following the death in November 1962 of his brother, Herman, George Brown became president of the Houston engineering firm. During this same period ownership of B&R passed to the Halliburton Company, a service company to the petroleum industry. Management of Project Mohole, however, continued under the direction of B&R. In September 1963 Herbert J. Frensley became president of Brown & Root with George Brown assuming the positions of board chairman and chief executive officer. (*Houston Post*, Sept. 22, 1963.)

When B&R became the prime contractor, however, Brown offered an olive branch. He did this by signing a contract with Bascom's new company, Ocean Science and Engineering, under which four of its members moved to Houston as part of B&R's Mohole staff. The arrangement turned out to be a brief and rocky one. Two months after beginning their work in Houston the OSE men resigned and returned to Washington. *Science* magazine quoted a B&R official as saying, "They had nothing to teach us." It quoted Bascom as retorting, "We had everything to teach them. They just didn't want to listen."[25]

Grateful for the contributions that Bascom and his assistants had made to the success of Phase I, NSF now took them on as project consultants. But this partnership, too, was shortlived. Bascom continued to make known his lack of faith in Brown & Root. After 10 uneasy months the Foundation terminated its contract with Ocean Science and Engineering, and Bascom and his aides bowed out of the Mohole picture.[26]

But these fretful developments still lay in the future when in December 1961 the second NSF evaluating group reported to Waterman. Its point rating confirmed those of the Preliminary Panel, and in a joint statement the two evaluating groups once again designated Socony Mobil's proposal as "in a class by itself." On January 15, 1962, the Foundation notified the seven lower-rated proposers that they had been eliminated, and the Preliminary Panel began a series of personal talks with representatives of the five organizations still under consideration. The upshot of these conferences was the issuance by NSF of a revised rating topped by the Gas Company (Global-Aerojet-Shell) with a numerical score of 968. Socony Mobil now occupied a close second with 964 points, followed by Brown & Root with 899, Zapata Off-Shore with 890, and General Electric with 846.

Following the elimination of Zapata Off-Shore and General Electric on February 1, four members of the Foundation staff inspected the offices of the three remaining contenders. On the heels of these site visits the NSF evaluating panels reassessed findings. Unable to reach complete agreement, they reported that all three organizations were "competent to effectively complete the Mohole Project." They did point out that the choice now lay between an engineering firm (B&R) on the one hand, and what were essentially two oil firms (Socony Mobil and the Gas Company) on the other, and that of the oil firms they preferred the Gas Company.[27] In the absence of a clear recommendation from his aides, Waterman made the final decision himself.

His selection of Brown & Root, announced on February 28, reflected the Director's belief that this $100-million-a-year company was strong in managerial know-how, a skill he regarded as taking precedence over all other considerations. In a memo for the Board he described B&R as having demonstrated an ability to complete "projects of great complexity . . . within and below cost and time estimates." The Houston firm's accomplishments included the construction of 240 offshore drilling platforms for oil companies, hundreds of combat vessels during World War II, and the 24-mile bridge over Lake Pontchartrain, near New Orleans. Waterman expressed confidence in the company's leadership, citing especially the cooperative attitude of George Brown.[28]

Stung by widespread objections to his decision, the Director defended it vigorously. A common claim was that Socony Mobil and the Gas Company had more in-house scientific and technological competence than the Houston engineering firm. Waterman's reply was that NSF and AMSOC would furnish the scientific guidance and that B&R could obtain by subcontract whatever specialized engineering capabilities it lacked. Another point raised by critics was that neither of the oil companies had asked for a fee. On the other hand, Brown & Root had requested one amounting to 4.5 percent of costs Waterman countered that it was "unbusinesslike" to contract without a fee. "The oil companies that did not want a fee," he said, "may have wanted something else. . . . They might have run after oil information instead of sticking to the scientific program . . ."[29]

Revealing of the Director's thinking is an incident involving Bill Benson. Summoned to a private talk in Waterman's office shortly before the selection was announced, Benson learned for the first time that it was to be Brown & Root. He also learned that in arriving at his decision Waterman has been influenced by his experiences during World War II at the Office of Scientific Research and Development.

"What the Director said to me," Benson recalls, "was that back in those Second World War days OSRD was letting research and development contracts for the production of new instruments and technological concepts, quite analagous to what we were asking of the companies interested in Mohole. And Alan said that when the OSRD people dealt with larger companies such as Socony Mobil or General Motors—not those specifically, but outfits of that magnitude—they usually had more trouble than when they dealt with a smaller company where there was direct access to the top boss and where that individual had a personal involvement in the problem. At the start of the job the larger company would say to the OSRD people, 'You let us have this contract, and a year from now we'll come back and deliver the whatsit to you.' If, 6 months later, OSRD looked at the job and said, 'Hey, that's not really coming out with what we want,' the big company would commonly reply, 'What you think you want is wrong. What you *really* want is this!' If OSRD insisted, a rumpus ensued. According to Waterman that sort of thing didn't happen with the smaller companies, at any rate not with the service-oriented ones—and that's what Brown & Root was, a service company."

Waterman was by no means Brown & Root's only defender. A sharp criticism of its critics came from Zapata Off-Shore, another Houston engineering firm and one of B&R's competitors for the prime contract. The *Houston Post*[30] quoted Zapata Off-Shore's president, George W. Bush, as predicting that Brown & Root "will probably do a good job." Bush added that an "injustice is being done the firm and the project when. . . [the attackers] just keep harping at it and have no constructive criticism." Bush attributed most of the troubles afflicting the project to the failure of members of the AMSOC Committee to think of themselves as "part of the team," adding that "they wanted to be captains." A similar thought was offered by Dr. William O. Baker, then vice president for research and now president of the Bell Telephone Laboratories and member of the National Science Board at that time. "Project Mohole," said Baker, "should not be damaged by members of any group who feel that they alone know the way to do things."

More distressing to the Foundation than the gripes about its prime contractor were published charges that politics had colored Waterman's decision. No evidence for these allegations was adduced then or later, but the circumstances on which they rested were open to inferences that it would have been professionally negligent of reporters to ignore. Houston, home of B&R, was close by the congressional district of Albert Thomas, chairman of the House subcommittee that passed on NSF budgets, and Thomas was an old friend and one-time college roommate of George Brown. Moreover, Brown and members of his family had contributed generously to the campaign funds of Vice President Lyndon B. Johnson. Simple fairness dictates the additional information that the Browns had been giving financial support to the Democratic party for at least two decades—a fact that had not prevented their engineering firm from getting other large government contracts, including one for $350 million, during the Republican administration of Ike Eisenhower.

Some critics said that at the very least it was "politically naive" of Waterman to choose a Texas company with LBJ in the vice presidency and Thomas head of the powerful appropriations subcommittee. But the charge of naivete might also have arisen had the contract gone to Socony Mobil or to the Gas Company. At the time the selection was made one of the firms associated with Socony Mobil in the Mohole venture was involved in a Federal tax action. As for the Gas Company, the parent company of one of its elements, Shell Oil, was a foreign corporation, based in the Netherlands.

Soon after the political charges surfaced, Republican Senator Thomas Kuchel of California asked Congress fiscal monitor, the General Accounting Office, to look into them. GAO's report was that "We are unable to conclude that the award to Brown & Root was not in the public interest."[31] In making his choice, Waterman, so far as can be determined, consulted with neither Thomas nor LBJ, nor any other elected official. Prior to the announcement of the award, Senator Allott, whose partiality for Socony Mobil has been noted,* had asked to be informed of the decision before it was made public. Waterman agreed, only to reflect later that to abide by such a commitment would set a bad precedent. Most observers got the impression that his failure to do so was a factor in Allott's outspoken and continuous opposition to Project Mohole after the announcement of a prime contractor.

The selection of Brown & Root in February 1962 was followed in March by the signing of a 90-day letter contract and on June 2[illegible] by the execution of a definitive cost-plus-fixed-fee contract.

The final document specified no deadline, but it was understood that B&R would try to finish Phase II in 5 years. It did offer a tentative estimate of future costs. This sum, $46.7 million, was based on a suggestion from the AMSOC Committee—a suggestion later dropped by B&R as impractical—that the Moho could be reached by a converted T-2 tanker to be donated by the Government. The figure, $46.7 million, was over two times higher than any previously mentioned in connection with the project. On the Hill, Democratic Congressman Joe L. Evins of Tennessee, Albert Thomas's second-in-command on the appropriations subcommittee, was heard to mutter, "pretty expensive digging,"[32] and the day was near when even higher cost estimates would prompt Senator Allott to describe Mohole as "a $100 million boondoggle."[33]

---

* Speaking on the floor of the Senate later on (*Cong. Rec.*, 89th Cong., 2d Sess., p. 18916) Allott pointed out that Socony Mobil was not located in his State and that so far as he knew the only Colorado-based company remotely connected with Project Mohole was a subsidiary of Brown & Root. Omitted from this statement was the fact that Socony Mobil did business in Colorado. The Senator's connection was not with the top command at Socony Mobil but with a regional officer. On learning of this situation the top officials of the oil company sent the regional officer to Venezuela and apologized to Brown & Root, pointing out that putting pressure on members of Congress was not their way of doing business. For a fuller picture of Senator Allott's dissatisfaction with the choice of Brown & Root, see ATW's draft diary note, February 27, 1962 ("Subject: Telephone Call to Senator Gordon Allott. . . ."), in the Waterman papers, LC.

B&R's first job was to develop an engineering plan, on the basis of which it would submit recommendations to NSF for completing the project. No one acquainted with the difficulties involved in Phase II expected the Houston firm to accomplish this task overnight. As Benson remarked, "With the completion of Phase I, we can be said to have flown the bi-plane at Kitty Hawk. Now we are asking the prime contractor to skip all the in-between steps and give us a jet transport." Quite likely the 11-month wait for Brown & Root to work up its recommendations would have evoked little concern within the Foundation and much less carping without, had all other aspects of the project been in good order.

But most of them were in disarray.

The sniping at Brown & Root went on. The political charges persisted, and as the months passed the already loosened ties between NSF and the AMSOC Committee began to separate at a rapid rate.

As has been indicated this development started shortly after the accession of Dr. Hedberg to the Committee chairmanship toward the end of 1961. Early in 1962 the new chairman reacted with anger on learning that Waterman intended to incorporate into his own staff a scientific officer for Project Mohole. On the grounds that all scientific counsel for the project should flow from the AMSOC Committee, Hedberg informed Academy President Bronk that if Waterman went ahead with his plans, he (Hedberg) might resign his post.[34] Although Waterman did go ahead, the Princeton geologist did not resign, and in an effort to clarify the relationship between the two groups, Waterman submitted to the AMSOC Committee a statement showing exactly where its responsibilities left off and those of the Foundation began. Accepted by the Committee on May 4, 1962, the agreement stipulated that "as in the past" the AMSOC group would "provide overall scientific design and guidance" but that the Foundation would make all final decisions.[35]

Concomitant with the execution of this written agreement, Waterman completed the establishment within the Foundation of an apparatus for riding herd on Phase II. To this end he arranged for an experienced engineer, C. Don Woodward, to set up shop in Houston as a liaison man between the prime contractor and NSF with the title of Project Coordinator. To deal with policy and administrative matters, the Director created a Mohole Committee consisting of Benson and two other Foundation executives, Paul A. Scherer (Associate Director for Administration), and Frank C. Sheppard (Waterman's Executive Assistant). A provision of this arrangement was that Benson would act also as scientific officer until such time as another scientist could be found to take over those duties on a fulltime basis. It was further understood that two other officials of the Foundation, Randal M.

Robertson (Associate Director Research) and Aaron Rosenthal (NSF Comptroller), would assist the Mohole Committee as needed.

Although the AMSOC people endorsed the division of responsibilities spelled out in the May 4th agreement, many of them found it hard to live with. Speaking as a member of both AMSOC and the National Science Board, geophysicist William W. Rubey of the University of California, Los Angeles, revealed that the AMSOC people felt that they were "being quietly pushed out of the Project by the staff of the Foundation."[36]

During a meeting in San Francisco, Calif., in the early spring, the members of the enlarged AMSOC Committee officially endorsed Chairman Hedberg's insistence that the emphasis of the project be shifted from the attempt to pierce the mantle to an intermediate drilling program and that an intermediate ship be acquired.[37] A few months later Hedberg took to the pages of a scientific periodical to explicate the reasons for this action. As his Committee had not yet got around to transmitting its March 1962 decision to the Foundation, Hedberg's article in the July-August issue of *GeoTimes* was the first signal to the agency that the AMSOC people had changed their minds about the objectives of Project Mohole.

The main point of Hedberg's hard-hitting article was that although his committee was "interested in the Mohorovicic discontinuity . . . the bulk of the world's geologists" were much more interested in the sedimentary rocks of the ocean crust above it. "One thing . . . in which AMSOC is not interested," Hedberg wrote,

> is in the promotion of a Mohole Project merely as a deep-drilling stunt. The mere fact of drilling deeper into the earth than anyone else without the expectation of an accompanying pay-off in scientific results has somewhat the same empty glory as that of having shot a missile farther into space than anyone else without any scientific equipment aboard. AMSOC is not interested in a race to the Moho which overlooks the worthwhile objectives which can be accomplished along the way.

Accompanying these words was a cartoon of two crows chatting on a fence. One was depicted as wondering what Mohole meant and the other as replying that it stood for "Mohorovicic discontinued."[38]

On September 7, soon after the publication of Hedberg's article, the growing strain between AMSOC and NSF came up for discussion at a session of the National Science Board. Waterman expressed regret that the Committee chairman had taken his opinions to the public instead of bringing them to the Foundation for private discussion. He said that in the near future the Foundation was planning to support precisely the sort of broad sampling of the oceanic crust to which the Princeton geologist attached so much importance. This activity, however, would not be funded as a part of Project Mohole; it would "be handled as a separate research under the Earth Sciences Program of the Foundation." As the Director would write later, he assumed that some intermediate drilling should be done before an attempt was made to plunge to the mantle. That, however, was simply his personal opinion. At that September 7th Board meeting he stated that the Foundation would make no decision about the intermediate program or the intermediate ship until Brown & Root had completed its engineering studies and was ready to offer recommendations.

The Director said that he was a little puzzled as to what the AMSOC people were "mad about." So far, efforts to find out had failed, partly because the members of NSF's Mohole staff had been away from the Foundation, partly because Hedberg, having hurled his thunderbolt, had gone abroad. Waterman voiced the hope that soon now "these misunderstandings" would be cleared up.

But the lines of communication between NSF and AMSOC had already become clogged beyond rapair. In the spring of 1963 Director of the Budget Kermit Gordon took cognizance of the troubles besetting the project: the rising cost estimates, the delay on the part of Brown & Root in completing its engineering plan, the now wide-open quarrel among the scientists as to methods and goals. On March 1 Gordon wrote Waterman

> that when this [second] phase of the project was brought initially to our attention, total costs of $15 to $20 million were anticipated. Last fall when when a request for $15 million was included in the budget for further funding, a total cost in the neighborhood of $50 million was discussed. Since then your [latest] congressional presentation . . . states that the Foundation regards $50 million as a minimum figure and that the ultimate total may be considerably higher.
>
> Given the financial as well as the technical uncertainties, together with the unique administrative problems involved . . . I believe the Foundation should withold its approval of further financial commitments . . . until the situation is clarified.[10]

Thus stood Project Mohole—its funds frozen—when in April Brown & Root delivered to both the Foundation and the AMSOC Committee a three-volume engineering plan along with the company's recommendations.

On the subject of an intermediate ship, the prime contractor said no. Brown & Root's argument was that the intermediate drilling could be done from the ultimate ship; that "there was nothing to be learned from a lesser machine that could not be better learned from using the vehicle specifically designed to" reach the Moho.

As for the ultimate vehicle: The company's experts had concluded that because of the problems created by heave neither a T-2 tanker nor any other single hull would do. What Brown & Root proposed instead was a 10,000-ton floating platform, 234 by 250 feet, resting on six 29-foot-high columns. Supporting the columns would be two submarine-shaped hulls, each 370 feet long and 35 feet in diameter. By means of propellers on the stern of each hull, the vehicle would take itself to the drilling site. There the hulls would be partly flooded. This action would bring the platform's displacement up to 24,000 tons and make it possible for drilling operations to proceed in near gale-force winds of up to 33 knots and in currents of up to 3 knots. Surrounding the vessel would be rings of sonar and radar buoys, both surface and subsurface. Signals from these would activate the propellers on the hulls along with others in the columns, thus holding the platform on its drilling station.

And the price tag? At this point the company's estimate was $67.7 million—$40 million for the platform, the rest to cover the costs of drilling to the Moho and the recovery of samples of the mantle rock.[40]

Although the company's recommendations were favorably received at the Foundation, all aspects of the project—including the question of an intermediate ship—were still under consideration there when at the end of June 1963 Alan Waterman retired and his successor, Leland Haworth, took over.

One of the new Director's earliest official acts was to get Project Mohole under way again. He did this on August 12 by persuading the Bureau of the Budget to release $2 million in Mohole funds so that the prime contractor could complete research and development plans and begin work on preliminary equipment designs.[41]

Near the end of Waterman's tenure, the National Science Board set up a Mohole review Panel under the chairmanship of physicist Emanuel R. Piore, IBM's vice president for research and member of the Board. In a final report, in October, the Piore panel endorsed Brown & Root's overall plans, simultaneously revealing that members of the panel held divided views as to the necessity for an intermediate ship.[42]

Meanwhile, in September, Haworth, in a meeting with AMSOC, had expressed the belief that a non-governmental scientific organization, analogous to the National Research Centers, should be sought to take overall responsibility for the program and that at some appropriate time Brown & Root should become a subcontractor of that organization. He had asked the AMSOC members to assist in finding or creating such an organization. He also had reaffirmed NSF's interest in sampling oceanic sediments and had indicated that the Foundation would try to support a sediment drilling program as a companion to Project Mohole. Many members of the Committee were delighted at both of these prospects, but Hedberg persisted in his view that any broader exploration of the sea bottom must be conducted as a part of the effort to reach the Moho.

The most crucial developments of that fall took place on the Hill. In late October the Subcommittee on Oceanography of the House Committee on Merchant Marine and Fisheries began hearings on the conduct of Project Mohole under the chairmanship of Democratic Congressman Alton A. Lennon of North Carolina. On the 29th Dr. Hedberg took the stand to say that "I'd rather see this project killed where it now stands than see it carried out . . . in a way which will not ensure that the country gets its maximum money's worth in scientific and engineering achievement."[43] The Princeton geologist reiterated his conviction that these ends could be met only by a large-scale intermediate program carried on by means of an intermediate ship—a statement that convinced Haworth that Hedberg and "some members" of his Committee had lost interest in piercing to the mantle and now regarded the intermediate drilling program as deserving of top scientific priority.[44]

Physicist Frederick Seitz had succeeded Detlev Bronk as president of the National Academy of Sciences. In a letter to "Dear Hollis," written immediately after Hedberg's congressional appearance, Seitz chided the AMSOC Chairman for presenting his "personal views concerning the course of the Mohole Project . . . as if they might be the official views of the Academy." Seitz wrote that whatever "the merits of your point of view may be, I feel it was improper to present such formal testimony to the Congress without clearing your proposed testimony with me so that I might have referred it to the Council of the Academy if I considered such a step wise." Seitz ended his letter with a warning: "I would like to inquire if you . . are prepared to assure me that henceforth you will not communicate with any organization or agency outside the Academy . . . on matters of major policy regarding the Mohole Project without first consulting me. . . . Unless you can assure me on this point, I will have no choice but to request the Council to permit me to reconsider your own status as chairman of AMSOC."

Replying on Academy stationery, Hedberg submitted his resignation, effective immediately. He called Seitz's letter "unfair and misleading," and asserted that "the stature of the National Academy and its reputation is vitally tied to the freedom with which its members may express themselves on matters of scientific import."[45]

With Hedberg gone from the Mohole scene, the departure of the AMSOC Committee became inevitable. Within a few months the

Committee had disbanded and the Academy had replaced it with two smaller groups of project advisors. Some press accounts of these developments cast the National Science Foundation in the role of villain of the piece. In *Science* and elsewhere, for example, Daniel Greenberg pictured NSF as having thrown the AMSOC people out, an action that he regarded as rank ingratitude in view of their contributions to the success of the first phase of Project Mohole.[46]

But was AMSOC the victim of unrequited love, or was it the other way around? Close to Project Mohole for several years following his appointment in 1962 to the National Science Board, the current president of the National Academy of Sciences, Philip Handler, has said that "as seen from the Board, the history of AMSOC was one of the sorriest episodes in the history" of the Academy. "They talked up the project," Handler says. "They sold it to the Foundation and guided the initial successes. Then they walked away. Granted the development of serious differences, perhaps even a fundamental flaw in the original program concept, by that time the Foundation was completely committed. It was most irresponsible. The Foundation was left with the contract but without responsible, committed, scientific leadership."

Not that Handler regards NSF's conduct of Project Mohole as a flawless performance. He faults the Foundation for having negotiated with Brown & Root a contract covering the whole of Phase II.

"I didn't learn about that," he says, "until well after I had joined the National Science Board. Then one day, at a meeting, I raised the questions, 'When is the preliminary phase over with? Do we get a preliminary report—cost estimates, etc.?' I was amazed to discover the contract had no such provisions, that it was an agreement to go the whole route. I then moved—and the other members agreed—that henceforth the NSF people should issue contracts for big jobs on a piecemeal or staged basis, starting with a feasibility study and cost estimate, thus giving them a chance at the end of each stage to review progress before going on."*

Although Handler now regards the nature of the contract with B&R as "the biggest blooper in the whole business," he attributes the failure of the project to the lack of what he calls "a Mr. Mohole." He points out that "from the beginning, unfortunately, there was no one major scientist in charge—no individual with his professional reputation on the line. The project was conducted by NSF and Brown & Root, and that was one hell of a way to do things!"[47]

---

* In a 1968 critique of the management of Project Mohole, the Comptroller General of the United States urged NSF to make staged contracts a matter of policy where large undertakings are involved. (U.S. General Accounting Office, *Administration of Project Mohole by the National Science Foundation*, Report to the Congress, April 23, 1968.)

Handler is not the first observer to deplore the behavior of the AMSOC Committee. In September 1963 a similar judgment was expressed by Robert C. Cowen, the natural science editor of the *Christian Science Monitor*. As it became clear that the Foundation and the AMSOC Committee were near a parting of the ways, Cowen wrote that the controversy between them "probably would never have arisen if the scientists who initiated the project had given it adequate supervision." Cowen conceded that this

> would have meant some sacrifice on their part of time for individual research. Yet, having obtained public funding for a major effort, such sacrifice surely is not too much for scientists to make. Without it, the project, however brilliantly conceived, is bound to suffer, as the Mohole story unhappily illustrates.

Cowen saw as crucial the decision of the AMSOC people, at the conclusion of Phase I, to turn over all responsibility for the project to the Foundation. At that point, he wrote, the

> AMSOC scientists realized that it would require more than parttime management to carry the project through its next and most challenging stages. . . Yet they did not want to devote that kind of time and attention to managerial and engineering work. The chore went by default to the National Science Foundation. . . .
>
> This was the root cause of the subsequent Mohole mess.[48]

The late fall of 1963 found the project leaders presenting their case to congressional bodies in an earnest effort to clear up the "mess." Speaking before Alton Lennon's oceanographic subcommittee in the House and NSF's appropriation subcommittee in the Senate, Haworth reiterated his belief in the need for a "*fulltime operating scientific* institution, such as a university, institute, or national laboratory" to take overall responsibility for the program. Neither the Academy nor the Foundation, was, or, in his opinion, should be such an operating organization, and Brown & Root, an engineering organization, made no claims in the scientific field.

With respect to scientific objectives, Haworth noted that the country's earth scientists were actually talking about three programs. Some were interested chiefly in drilling through the sediments on the ocean floor, a relatively easy task susceptible to being performed with a small ship. Others wanted to core deeper in an effort to explore "the so-called second and third layers" of the oceanic crust. Still others were primarily concerned with drilling to the Moho.

Endorsing all of these objectives, the NSF Director said that "if funds were no object—a strong scientific case could be made for a three-ship approach." But, "since, in fact, costs *are* important and funds *are* limited it is necessary to work out a satisfactory program in this important scientific field which takes advantage of progress (in drilling technology)— and which will assure the best scientific results over the years within reasonable costs." Deeming it "a mistake to speak of the Mohole Project as one that can be definably bounded in cost and time" he asserted that "clearly, once obtained, the facilities will actually be used continuously and indefinitely for scientific purposes," that is for drilling deep into the crustal layers including one or more penetrations to the Moho. Hence, "in terms of cost and schedule, one should think of a facilities package of a capital nature, plus a continuing research program funded on an annual basis for an indefinite period," rather than of a single one shot venture.

As to the need for an intermediate drilling program to help develop the technology, there was, Haworth said, universal agreement. Only the need for an expensive intermediate vessel was in controversy. Complicating the issue was the fact that such an intermediate ship would at some time become available for other tasks, not related to the Mohole drilling proper, thus making it especially attractive to those primarily interested in the shallower drilling.

Though regretting, in "hindsight," "that the work of Brown & Root was not paralleled by a continuous drilling program directed at both the development of equipment and techniques and at acquiring useful scientific information" Haworth pointed out that meanwhile the development of the submerged hull drilling platform concept had developed rapidly, that it was strongly endorsed by the AMSOC Drilling Techniques Panel, and that Brown & Root had reached a point warranting initiation of detailed designs for a fullscale Mohole platform. Hence progress toward the ultimate Mohole platform "would be seriously delayed by the intervention of an intermediate ship." Furthermore, to meet the developmental objectives, an intermediate vessel must have a deep drilling capability, and hence be quite expensive.

Concluding that there should be two different but complementary drilling programs, one for deep and the other for sedimentary and upper crustal drilling, Haworth proposed, "for the Mohole program proper" to omit an intermediate ship, but rather to build the large platform recommended by Brown & Root, to equip it initially for only an intermediate drilling program, and, once experience was gained, to augment the equipment to the extent necessary to undertake the mantle penetration. This would, he said, provide a very stable platform for the intermediate development program, provide a facility for intermediate and deep drilling for scientific purposes indefinitely, and avoid the expense of a second deep drilling vessel, thus helping to minimize the cost.

Continuing, he said "I also favor and am prepared to recommend a supplementary drilling program, not part of Project Mohole," but under AMSOC purview, that would utilize a much smaller vessel "for drilling in the unconsolidated sediments and into the immediately adjacent rock. . . . In addition to acquiring scientific knowledge such a program would permit technical developments leading toward a deeper drilling capability" so that "ultimately, perhaps, one might hope to have a vessel equipped to drill, say, through the second and into the third layer of the earth's crust." The Mohole vessel could, of course, cover all depths beyond the smaller vessel's capability. Hence, one could look forward to meeting all the foreseeable objectives.* [49]

NSF's efforts to put Mohole's house in order were temporarily effective. Early December brought word from the Office of Congressman Lennon that the testimony heard by his subcommittee had convinced him that the project was being properly conducted. A few weeks later the Bureau of the Budget lifted all restrictions on project funds.

On January 21, 1964 Haworth announced that Phase II would be a two-stage, one-ship affair. He instructed those in charge to proceed at once "to the design and construction of the large stable . . . platform" suggested by Brown & Root. As for the intermediate program, his instructions were that "initially" the platform would be used "with less than its ultimate equipment, to drill several holes to intermediate depths in order to acquire operational experience and engineering information [along with] . . . samples of the [oceanic] crust before tieing it up for 2½ to 3 years while drilling a single hole to the mantle."[50]

Exactly 1 year and 6 days after Haworth's decision to go the two-stage, one-ship route, the Foundation announced that the Mohole would be drilled in the Pacific Ocean 115 miles northwest of Maui in the Hawaiian Islands. Selected by one of the Academy's project committees, the site was described as "at coordinates 22° 22′ north latitude, 155° 28′ west longitude: Water depth, approximately 14,700 feet; estimated depth to mantle from 29,000 to 32,000 feet." In its January 27, 1965 announcement of the Mohole site, the Foundation also revealed that seven other places in the Pacific had been chosen for experimental drilling to lesser depths prior to the major effort. "Thus," Haworth pointed out in a subsequent report to the White House, "we will accomplish another objective of the Project, that of drilling into the second and third layers of the crust at several different points."[51]

---

* At a meeting of the National Science Board on November 21, 1963, Haworth reviewed for the members the proposals he had made to the Congressional subcommittees. For "the Mohole program proper" he proposed the construction of a large platform such as Brown & Root had recommended. "(P)rovide it initially," he said, "with something less than the ultimate drilling equipment, to carry out an intermediate program, developing both equipment and techniques through this experience and . . . proceed in an orderly manner toward the capability required for deep drilling . . . to pierce the mantle; in other words, . . . let the large platform serve as the basic intermediate ship." Haworth also recommended what is now NSF's Ocean Sediment Coring Program, described by him as "a supplemental drilling program, not part of Project Mohole, utilizing a much smaller vessel." The Board approved both recommendations unanimously. (NSB-ES 90, p. 1, and see "The Geological Revolution" in Chapter 16 of this book.)

At this time there was reason to believe that the project would go through. In point of fact its most serious woes lay just around the corner.

During the opening months of the year Brown & Root invited proposals for the construction of the big Mohole drilling platform. Accepted by the Foundation on September 28, the lowest bid received—that of the National Steel and Shipbuilding Company of San Diego—was $29.9 million. This was $11.6 million more than NSF had planned on spending for the platform. Along with increases for other components it lifted the estimates of total overall costs from $90 million (the last more or less official figure to emanate from the Foundation) to about $127 million—a jump that did not go unnoticed among members of a Congress coping with the financial burdens of the Vietnam War.[52]

The withdrawal of the AMSOC Committee had quieted the long wrangle over goals and procedures among the scientists; but the memory of that dispute was still around when Project Mohole lost its most powerful supporter in Congress with the death of Albert Thomas on February 15, 1966. His successor as chairman of the House appropriations subcommittee, Joe Evins, had long since made known his impatience with the rising costs of the deep-drilling effort. Both on the Hill and at the Foundation, moreover, it was common knowledge that relations between Evins and his predecessor had been less than cordial. For advice on important matters Thomas had tended to go to lesser-ranking members of the committee, often by-passing his second-in-command in doing so. Few if any knowledgeable observers were surprised, when at the conclusion of the spring budget hearings, Evins' committee said no to NSF's request for $19.7 million to carry Project Mohole through the forthcoming fiscal year.

For a few weeks it looked as though the Senate might succeed in salvaging the project, but in the end the efforts of a handful of Mohole supporters were unavailing. When in the late summer the FY 1967 appropriations measure went to conference, the Senate deferred to the House—and the final bill, passed on September 6, 1966 specifically forbade the Foundation from applying any of its funds to Project

Mohole. At this point the total funds obligated for the project (exclusive of Phase I) stood at about $55 million, of which $24 million had been expended. During subsequent termination procedures, $28.9 million was deobligated for use by the Foundation for support of its other activities.*

Benson of NSF was out of the country when on that fall day in 1966 Project Mohole collapsed. He was in Tokyo, attending sessions of the Pacific Science Conference. When the news arrived there, a number of foreign scientists hastened to offer their condolences.

Among them was Vladimir V. Beloussov, a top man in Soviet geology and past president of the International Union of Geodesy and Geophysics. It was Beloussov who years earlier had called his government's attention to Project Mohole, pointing out that as the Americans were trying to reach the mantle the Russians ought to be trying it, too. The Soviet government had responded by funding studies looking forward to an attempt to do so by land—plans that never materialized because of the government's refusal to go beyond the study stage.

Benson's recollection of the conversation in Tokyo when Beloussov came around to commiserate with him runs as follows:

> Beloussov: Well, Dr. Benson, this is a sad day, but I've got more reason to be sad about the Mohole than you have. You've got only one reason, and I've got two.
>
> Benson: Oh, come on, Vladimir.
>
> Beloussov: Well, you're sad because this very fine project has been canceled, and for good scientific reasons you are very sad. I'm sad for all those reasons, but in addition, . . . I must now go back to Moscow, and my politicians are going to say, "If rich America can't afford the Mohole, why should we give you any more money for deep drilling on the continents?". . . .After all, you used our Sputnik to get money

---

* C.R. 90.9 (1967), pp. 185-187, gives the following information concerning costs, obligations, and status of material at termination of project:

Phase I: Total cost from 1958-63. . . . . $1,810,183

Phase II: Funds made available, Fiscal Years 1962-64, were $54,630,478, of which about $24 million had been expended as of May 1, 1967, leaving at that time about $30 million of the obligated funds unspent. A chart on page 187 shows that most of the 20 major items of equipment already fabricated for the project were held for use in today's Deep Sea Drilling Program (see next chapter) with some items going to the Naval Civil Engineering Laboratories for use in studies of ship salvage and for general ocean engineering. As for the big Mohole platform and its associated elements, a note reads that "little or no fabrication was done—all design studies available, and any material available suitable only as scrap." The model of the big Mohole platform can now be seen at the Smithsonian Institution.

for your space program. Too bad, you couldn't have given us Mohole so that we could get money for good scientific drilling, and then we'd both be happy.

But, of course, Mohole was finished. Not dead—to say as much might be going too far. Valid scientific inquiries rarely if ever die. It stands to reason that someday, somewhere, someone will dig that big hole.

Pending that happy day an NSF-funded activity, widely regarded as a direct outgrowth of Project Mohole, has earned historical laurels for itself as one of the most productive scientific developments of the present century. This is the general drilling program that Haworth, in the fall of 1963, had officially recommended.

On the records of the Foundation it is listed as the Ocean Sediment Coring Program. Often, however, one hears it referred to as JOIDES for the reason that the program receives its impetus and its scientific guidance from a consortium of marine research centers called the Joint Oceanographic Institutions for Deep Earth Sampling.

JOIDES is also spoken of as "the poor man's Mohole," and very likely many observers would agree with a thoughtful comment on its origins by geophysicist Jack Oliver, whose contributions to the theory of plate tectonics have been mentioned in these pages.*

Pointing out that he was "never directly involved in the Mohole Project," Oliver adds that "I have always felt, however, that if you could leave aside some of the political things and some of the false starts that were made, Project Mohole could be regarded as a good thing in that it stimulated a lot of interest in deep drilling. In that sense it was essentially the originator of JOIDES, which has been an extremely successful program."[53]

The remote beginnings of JOIDES have already been alluded to. Early in 1961 the success of the first phase of Project Mohole—the series of operations in the Pacific showing that holes could be drilled in the ocean floor under open-sea conditions—prompted Alan Waterman to pose a question to Gordon Lill, then chairman of the AMSOC Committee.

"As we approach the successful conclusion of the first phase of our program," the NSF Director wrote on April 19, 1961, "should we give consideration to the desirability of a survey of the upper crust under some appropriate ocean areas? Since our . . . commitment [to Project Mohole] is already of some magnitude, I do not wish to imply that we are now ready to involve further funding. However, it seems possible that the scientific values to be gained from ocean floor drilling may even exceed those associated with drilling to the discontinuity."[54]

---

* In chapter 1.

By the time Waterman retired from the Directorship and Haworth succeeded him, the conduct of Project Mohole had been placed under Brown & Root of Houston, Tex. By this time, too, most of the AMSOC people were insisting that a sediment coring program—meaning an extensive program of shallow drilling in the ocean floor—be carried out as part of the effort to pierce the Moho.

As previously related, Haworth, like his predecessor, saw the value of such an undertaking. Study of the situation as he found it in the summer of 1963, however, convinced him that the proposed sediment drilling program should be organized as a separate entity and not be tied to the Mohole venture. One of his reasons for this conclusion was that, as an engineering firm, Brown & Root made no claims to the scientific knowledge that the supervision of a large-scale sampling of the oceanic crust would require.[55]

Even before Haworth took over, several oceanographic institutions—and in one case a group of individual experts—had offered to set up consortia for the direction of a sediment drilling program. Two preliminary attempts to this end had fallen through, chiefly because the proposed managerial groups were too loosely knit, but the idea itself had persisted. Haworth liked it. His long connection with Associated Universities, Incorporated, managers of an AEC laboratory and an NSF observatory, had convinced him of the efficacy of such arrangements. Indeed, he had suggested to AMSOC and to various individual scientists that a consortium would be a quite appropriate manager for Project Mohole and had urged them to try to catalyze some appropriate arrangement. Following his testimony before congressional committees during the fall of 1963 announcing his intention of setting up a sediment coring program whenever funds could be made available, Haworth attempted to have initial funds included in the FY 1965 budget, then in the final stages of preparation. Although accepting the project in principle, the Bureau of the Budget did not recommend funds until the following year. During the same period Haworth got in touch with the AMSOC Committee, expressing the hope that its members would offer suggestions for the creation of a proper managerial setup. When, after much discussion of this request, the AMSOC people took no definite action on it, the Director and his aides proceeded on their own.[56]

May 1964 saw the formation of the Joint Oceanographic Institutions for Deep Earth Sampling. Organizationally JOIDES is not a copy of Associated Universities, Incorporated, which is an operating corporation. Following a suggestion made by Haworth it is modeled rather after the agreement under which Harvard and MIT jointly ran the Cambridge Electron Accelerator on the Harvard campus. All members of JOIDES participate in the scientific aspects of the program. For each specific project, however, only one of them serves as the contractor in charge of operations. In actual practice numerous institutions not connected with JOIDES take part in the work, as core samples acquired during drilling activities are distributed to qualified investigators throughout the Nation.[57]

Originally JOIDES consisted of four research centers: Lamont-Doherty Geological Observatory, Woods Hole Oceanographic Institution, University of Miami, and Scripps Institution of Oceanography of the University of California at San Diego. In 1966 the University of Washington joined the group. In 1973, as a result of President Nixon's visit to the Soviet Union the year before, the Institute of Oceanology of the U.S.S.R. became the sixth member, and more recently the Federal Republic of Germany has become the seventh.

An unexpected opportunity enabled the JOIDES program to get started months before any money was formally allotted to it. In the spring of 1965 geologists at Lamont-Doherty received an offer from an American oil company. The company was about to send a drilling vessel, the *MV Caldrill*, from the West Coast to the Atlantic to do some work off the Grand Banks of Newfoundland. Such was the schedule of the job that the *Caldrill* was going to have about a month of dead time. The Lamont-Doherty people approached the oil company which indicated that it would pay the moving costs of the vessel even if it were used by the JOIDES group during the move. It was capable of drilling in water of up to 6,000 feet, so drilling could be carried out in moderate water depths anywhere between California and Newfoundland.

At Haworth's suggestion the Lamont-Doherty geologists got in touch with the rest of the JOIDES group, and plans for a drilling operation off the coast of Florida were quickly arranged. Lamont-Doherty acted as operating contractor. NSF dug up $250,000 from its regular basic research funds to take care of running the *Caldrill*; and for a few weeks beginning in April 1965 the JOIDES scientists carried out drilling operations along the continental shelf and on the Blake Plateau.

Brief though it was, the enterprise yielded useful results. It taught the JOIDES investigators how to function as a team and enabled them to work out the logistics of a complicated maneuver at sea. Scientifically it provided them with some partial answers to questions concerning the properties of the continental shelf and the Blake Plateau. On the practical side it demonstrated that, by means of offshore drilling, the fresh water in the aquifers (underwater artesian wells) some 50 miles from the coast of Florida could be recovered and piped to land.

In 1966, with a starting budget of $5.4 million, the sediment coring program began in earnest when in September of that year the Foundation accepted a proposal from the Scripps Institution of Oceanography for what a few months later became the now famous Deep Sea Drilling Project. Principal tool of this venture is the *Glomar Challenger*, a 400-foot drilling vessel operated for JOIDES by its owners, Global Marine, Inc. By 1974 the *Glomar Challenger* had operated extensively in three oceans and its crews had brought up hundreds of sediment cores. Now scheduled to continue through 1976, the Deep Sea Drilling Project has produced an impressive array of results, ranging from the discovery of oil and mineral deposits in the Gulf of Mexico to the acquisition of persuasive evidence of several kinds in support of important aspects of the theory of plate tectonics.

Chapter 11. Project Mohole and JOIDES

[1] Jerome R. Ravetz, "The Mohole Scandal," *The Guardian*, Oct. 27, 1964.

[2] Daniel S. Greenberg, *The Politics of Pure Science*, New York, 1967, p. 171.

[3] Herbert Solow, "How NSF Got Lost in Mohole," *Fortune*, May 1963, p. 138.

[4] *Business Week*, Nov. 23, 1963, p. 32.

[5] *Fortune*, April 1964, p. 106.

[6] NAS-NRC, AMSOC Committee mimeographed memo: William L. Petrie, Executive Secretary, to Members, Subj: AMSOC-Mohole Chronology . . . 1963 (cited hereafter as AMSOC Chron). On the pre-Mohole drillings, see Willard Bascom, *A Hole in the Bottom of the Sea*, Garden City, N.Y., 1961, pp. 35-36 (cited hereafter as Bascom).

[7] Bascom, pp. 46-47.

[8] Interview; AMSOC Chron.

[9] AMSOC Chron; Bascom, pp. 49-51; Brown & Root, Inc., *Project Mohole: Phase II* (unpublished history prepared for NSF), n.d., p. 2 (cited hereafter as B&R Mohole History).

[10] ATW, memos to NSB, May 16, 1958, Mar. 3, 1959, Chron File.

[11] *Mohole Project*, Hearings before the Subcommittee on Oceanography of the [House] Committee on Merchant Marine and Fisheries, 88th Cong., 1st Sess., Washington, 1963, p. 162 (cited hereafter as Marine Committee Hearings 1963).

[12] AMSOC Chron; NAS-NRC, AMSOC Committee, Publication 717, *Drilling Through the Earth's Crust*, Washington, 1959; John Steinbeck, "High Drama of Bold Thrust through Ocean Floor," *Life*, pp. 111-122; NSB Min. 60:6; and in Chron File: ATW, memos to NSB, May 10 and May 13, 1960, Jan. 9 and Aug. 17, 1961; letters to Bronk, Nov. 8, 1960, to George B. Kistiakowsky, Dec. 1, 1960; diary notes covering phone calls to Admiral Rawson Bennett, Arp. 8, 1960, and Harold Gerskinowitz, Dec. 6, 1960.

[13] Gordon Lill to Dr. Edward B. Espenshade, Jr., June 14, 1961, enclosing AMSOC Committee's recommendations for future of Project Mohole (cited hereafter as AMSOC Guidelines).

[14] AMSOC Chron.

[15] Minutes of AMSOC Committee meeting of June 13, 1961, quoted in Marine Committee Hearings, 1963, p. 171.

[16] AMSOC Guidelines.

[17] In Chron File: ATW memo to NSB, Aug. 17, 1961; ATW to Rep. Charles M. Teague, June 4, 1962; LJH to Senator John G. Tower, Nov. 4, 1963 (attaching June 18, 1962 report on Mohole by Comptroller General of the United States to Senator Thomas H. Kuchel).

[18] B&R Mohole History, pp. 12-35.

[19] ATW, Memo to Timothy J. Reardon, Jr., Special Asst. to the President, Dec. 18, 1961.

[20] B&R Mohole History, p. 286; AMSOC Chron.

[21] Chron File, Oct. 18, 1961.

[22] ATW diary note, Feb. 1, 1962, Chron File.

[23] ATW diary note, Feb. 14, 1962, Chron File.

[24] AMSOC Chron.

[25] Daniel S. Greenberg, "Mohole: The Project that Went Awry," *Science*, Jan. 17, 1964, p. 226.

[26] Richmond, Va., *Times-Dispatch*, June 1, 1963; *New York Times*, May 31, 1963.

[27] Interviews with NSF officials.

[28] Chron File, Mar. 8, 1962.

[29] ATW to Herbert Solow, Dec. 31, 1962, Chron File.

[30] June 14, 1963.

[31] Comptroller General of the United States to Kuchel, June 18, 1962.

[32] B&R Mohole History, p. 51.

[33] Ibid., p. 192.

[34] Ibid., pp. 290-293; Marine Committee Hearings, 1963 (Haworth's testimony on Nov. 12).

[35] ATW to Hedberg, May 11, 1962, Chron File.

[36] NSB-ES 83:6.

[37] Marine Committee Hearings, 1963, p. 190; B&R Mohole History, pp. 287-288.

[38] Vol. VII, No. 1, pp. 8-11.

[39] Attached to ATW to Gordon, Mar. 15, 1963, Chron File.

[40] B&R Mohole History, pp. 165-180; NSB-ES 85:3; ATW to Senator Clair Engle, June 11, 1963, Chron File.

[41] B&R Mohole History, p. 296.

[42] NSB-ES 88:1-2.

[43] Marine Committee Hearings 1963, p. 40.

[44] LJH to the Rev. Theodore M. Hesburgh, Nov. 15, 1963, Chron File.

[45] B&R Mohole History, pp. 365-369; *Business Week*, Nov. 23, 1963, p. 32.

[46] Daniel S. Greenberg, *The Politics of Pure Science*, New York, 1967, p. 204.

[47] Personal interview.

[48] "How Mohole Bogged Down," *Christian Science Monitor*, Sept. 18 and 20, 1963.

[49] Marine Committee Hearings 1963 (Haworth's testimony on Nov. 12).

[50] B&R Mohole History, pp. 309-310; LJH, memo to William Moyers, Assistant to the President, Mar. 17, 1964, Chron File.

[51] LJH memo to Jack Valenti, Special Assistant to the President, Feb. 16, 1965; B&R Mohole History, p. 380.

[52] In Chron File: LJH, memo to Jack Valenti, Special Assistant to the President, Aug. 17, 1965, and LJH, telegram to NSB, Sept. 28, 1965; U.S. General Accounting Office, *Administration of Project Mohole by the National Science Foundation*, Report to Congress, April 23, 1968, p. 9.

[53] Interview.

[54] Chron File.

[55] LJH to Senator Warren G. Magnuson, Jan. 21, 1967, Chron File.

[56] Ibid.

[57] Ibid.

Chapter 12

Revising the Charter

When on July 18, 1968 President Lyndon B. Johnson signed into law a bill numbered H.R. 5404 he created what is now often referred to as the "second NSF charter." Its official title: *Public Law 90-407, an Act to amend the National Science Foundation Act of 1950 to make changes in the organization and operations of the Foundation, and for other purposes.* Its popular title: the Daddario bill after Democratic Congressman Emilio Q. Daddario of Connecticut, chairman of the Subcommittee on Science, Research, and Development of the House Committee on Science and Astronautics where the measure took form.*

During the 18 years preceding the passage of the Daddario bill Congress had tinkered very little with the original NSF charter. Most of the consequential changes in the workings of the agency had been effected by Presidential directives in the form of Executive orders and reorganization plans. When in the early 1960's, therefore, the Daddario subcommittee initiated the first full-scale effort to renovate the scope and structure of the Foundation, it served notice that in the eyes of its members NSF had become too vital to the well being of the country to be managed exclusively by executive ukase.

That the subcommittee regarded itself as breaking ground in this respect was brought out in one of its reports to the House of Representatives. According to this document its members put no stock in the view that "the steps necessary to a vitalized . . . Foundation can all be taken without congressional action—or that in the absence of . . . legislation, they will all be taken administratively. . . Legislative intiative is a crucial part of the political system by which our National Government functions. If, in recent years, it has been relegated to the sidelines—as some contend—perhaps the issue at hand [the refurbishing of NSF] is an indication of a reverse trend."[1]

---

* In its final months of consideration, it would become the Daddario-Kennedy bill, recognizing the role of Senator Edward M. Kennedy of Massachusetts who sponsored the bill in the Senate and guided its passage there, and subsequently would become Chairman of the Foundation's authorization subcommittee in the Senate.

Of all the changes dictated by the new charter none had invoked as much discussion as a brief clause aimed at enlarging the Foundation's operational powers. Prior to the Daddario bill NSF can be said to have had only two major operational functions. Those, as we have seen, were to promote research and education in the basic sciences. The Daddario bill attached another string to its bow by allowing NSF to foster applied research as well. The second charter did not say that the agency had to support work of this type, only that it could do so whenever its leaders thought it should, with one reservation, namely, that grants for work of this sort could be made to profitmaking firms only at the direction of the President of the United States.

Unlike the original enabling act, the new charter stirred none of the open controversies that had so often punctuated and sometimes impeded the legislative development of the first one. Reading the transcripts of Congressional hearings on the various Daddario bills one is struck by the sweet reasonableness of most of the testimony.

What makes this situation noteworthy is that the clause permitting NSF to sponsor applied research was not greeted with hosannas in the scientific world. Well conned in those quarters was Vannevar Bush's much-quoted expression of science's Gresham's Law—namely, that applied research tends to crowd out basic research. That many American scientists found the legal sanctification of applied science a bitter pill to swallow is evident from the record. That they did so with a minimum of grumbling can only be understood as a tribute to Congressman Daddario. In truth, it is hardly farfetched to suggest that the legislative career of the second charter would have been a far stormier one had a man of lesser stature been in charge of the bill.

A lawyer by profession, "Mim" Daddario, as friends call him, experienced his first brush with science during his undergraduate days at Wesleyan University in Middletown, Conn., during the late 1930's. Later, as an officer with the Office of Strategic Services in World War II, he was able to observe at first hand the importance of scientific expertise to the winning of that struggle. Coming to the House in 1959 as the representative of the first Congressional district of Connecticut, he had proceeded to demonstrate an active and intelligent interest in the welfare of the American scientific community that quickly won him the respect and trust of its members.

When in 1970 Daddario left Congress to make an unsuccessful run for the governorship of his State, Daniel Greenberg marked his departure with a curious article in the September 25th issue of *Science*. One of the reporter's assertions was that the retiring Congressman's championship of the scientific community had been "a rare act of political altruism . . . for it is hard to see what he himself got out of it . . . and it is easy to see how he might have derived greater political benefit" by going along with "the public disenchantment with science" then in vogue. In a subsequent issue of the magazine, on September 18, 1970, physical chemist Franklin A. Long of Cornell University (whose controversial nomination to the directorship of NSF the year before will concern us later) took issue with Greenberg's characterization of Daddario as "an irrelevant" politician. That, in the opinion of Professor Long, was a singularly inept description of "a fine and able man."*

When today Daddario is reminded of the fuss occasioned by his proposal in the mid-1960's to add applied research to the NSF mandate, he nods thoughtfully. "Yes," he agrees, "that was the emotional issue. But at the time Congress was grappling with monumental national problems, and it was plain that if the Foundation continued to limit itself to only certain kinds of research, it was going to have a hard time getting adequate budgets. Obviously it needed broader powers."

His immediate reaction to a request to comment on how the Foundation has handled its new powers is a rueful smile. "The fact that this year [1974] NSF is spending 23 percent of its research funds on applied research," Daddario says, "suggests that it may be going too far in that direction. The intent of the 1968 law was not to weaken the Foundation's support of basic research but to bolster it."[2]

A provocative response, certainly, but to dwell on the effects of the Daddario bill at this point would be to get ahead of our story. The next chapter will concern itself with NSF's efforts to implement the bill. This one will concentrate on the bill itself, its philosophical underpinnings, its contents, its travels through the legislative thicket. As has already been indicated, one scans the record in vain for the open disputes that often dramatize the progress of a congressional measure. But if the account of the second NSF charter cannot be billed as the battle of the ages, it can be called instructive. No one movement in the history of the Foundation better illustrates both the strength and the fragility of the ties that bind the agency to its four "publics": the scientific community, the other elements of the executive branch of the Government, the Congress, and the people whom the members of that tribunal represent.

---

* Dr. Long also took note of an ambivalence in what he described as Greenberg's "verbal overkill." Having faulted the Daddario bill as a plot to get more Federal money for science, Greenberg wound up his article with an expression of regret that the Connecticut lawmaker had not been able to do more for it in that regard. Daddario's desertion of the Washington scene, incidentally, was temporary. He now heads the Office of Technology Assessment, a recently created arm of the Congress.

As a prelude to an examination of the steps leading to the enactment of the Daddario bill, a few words are in order about the social and political backdrop against which they occurred. Scientists were popular folks in the 1950's—too popular in the opinion of some observers who saw cause for alarm in what seemed to be a tendency on the part of the American citizenry to expect too much of science and to regard its practitioners, almost idolatrously, as latterday miracle workers. As the tensions of the dissonant 1960's took shape, however, these attitudes described an almost 180-degree turn. Into their place came for a time at least what seemed to be a broadly felt disapproval of everything even remotely scientific. This sentiment was especially virulent among the new-left radicals on the campuses of higher education, prompting Don Price of Harvard to remark that in the minds of countless American youths science had become an object "of fear and hostility" and its major offspring, technology and material progress, "the gods that failed."[3] If during the 1950's Americans often asked, "what can we do for science?" in the 1960's their main question was, "what can science do for us?" Pollution in the country's lakes and streams, distress and decay in its slums, crime on its streets, breakdowns in its transportation systems—these and many other national discontents clamored for remedy.

It was in the midst of this bewilderingly shifting climate of opinion that the second NSF charter moved to fruition. Its beginnings go back to the 1950's when escalating Federal expenditures for scientific research and development necessitated the formulation by Congress of special entities to provide legislative oversight of the activities involved. One of these bodies, established in 1959, was the House Committee on Science and Astronautics, chaired by Democratic Congressman George P. Miller, a civil engineer from San Francisco, Calif. NSF was within the purview of the Miller group, but in the beginning the Committee was unable to give the activities of the agency more than a passing glance. As the Chairman revealed, the main reason for this was that for some years the problems connected with launching the American space program consumed "an inordinate amount" of his Committee's time. When time did become available in 1963, NSF was undergoing a transition, with Waterman stepping down and Haworth taking over, and to the Science and Astronautics Committee Chairman "it appeared best not to begin a general review of the Foundation until the incoming group had a chance to get its bearings. . . ." Explaining these delays at the opening of a congressional hearing in 1965, the California congressman added that "nonetheless almost 3 years ago, Mr. Daddario and I began discussing the advisability of a thorough review of the Foundation's mission" and that Daddario had then agreed "to chair a subcommittee to undertake the task as soon as the proper moment arrived." The moment arrived toward the close of 1963, at which point the Daddario subcommittee came into being and the investigation of the Foundation began.

For well over a year the new subcommittee and its staff contented themselves with gathering information. At their request the recently created Science Policy Research Division of the Library of Congress prepared a comprehensive survey of the programs and growth of the Foundation during its first 15 years. Testimony taken at executive (closed) hearings provided them with the suggestions and views of other science-using Federal bureaus and of industrial researchers. Conferences with a study panel of the National Academy of Sciences gave them a picture of how much basic science the country appeared to need and of what phases of it ought to be intensified.

These accumulated data convinced them that to fashion a suitable new mandate for NSF they must look for the answers to three questions:

> "(1) How well has the National Science Foundation performed the tasks which Congress set out for it in the basic act of 1950?
>
> "(2) What roles, responsibilities, and missions should the . . . Foundation assume in the future?
>
> "(3) What tools will [it] . . . need in the years ahead to accomplish its work?"[4]

At public hearings in the summer of 1965 spokesmen for the Foundation, for other science-oriented Federal bureaus and for the American academic and industrial communities addressed themselves to those questions. Beginning on June 23, the sessions continued intermittently for 7 weeks with Director Haworth taking the stand on two occasions as the first and last of 41 witnesses in all.

On the heels of these hearings, the Subcommittee engaged in another round of executive sessions and study periods. By February 1966 it had published its findings in a detailed report entitled *The National Science Foundation: Its Present and Future.* By spring of that year it had drafted a bill embodying its recommendations for enlarging the authority and upgrading the leadership of the Foundation—recommendations that, with only a few changes and additions, would survive 2½ years of legislative pulling and hauling to become law in 1968.

At the opening of the 1965 public hearings Chairman Miller of the full Committee had announced that the inquiry into the practices and policies of the Foundation was intended to be a critical one. Although *Its Present and Future*—to give the Subcommittee's 1966 report a convenient name—bore out this statement, it emphasized that the word "critical" did not mean that the investigators were "pointing fingers at failure, mismanagement, or nonfeasance." It seemed to them that in general the Foundation had turned in a fair performance, and that most of its shortcomings were the outgrowth of circumstances beyond its control. One such circumstance was the swiftness with which NSF had grown—a phenomenon measured by the jump in its appropriations from $3.5 million during its first operating year (1951) to $420 million in 1965. Another lay in the even greater speed with which the problems of the country had multiplied in recent years. As science and technology would be called upon to assist in the solution of these difficulties, the subcommittee's "cardinal criticism" of the Foundation was that it "is operating, and is largely organized to operate, in a manner which was satisfactory a decade ago but which does not appear adequate for either today or tomorrow."[5]

Expanding on this thought, the authors of *Its Present and Future* cited what appeared to be four paramount deficiencies in the conduct and attitudes of the Foundation.

One concerned the range of the agency's research-support activities. Too narrow, according to the legislative critics. The subcommittee said that the growing acuteness of certain national needs—among them "a clean world to live in" and a "population in balance with resources"—demanded that in the future NSF's programs "reflect the interests and requirements of industry . . . as well as the [those of] the academic community." The part of the second charter that now provides for the support of applied research reflects this thinking. On its own NSF is now free to foster such activity at any academic or other nonprofit institution. In addition, at the direction of the President and in the case of "research relevant to national problems," it may also do so "through other appropriate organizations," including profitmaking industrial firms.

Another complaint stressed by *Its Present and Future* dealt with the Foundation's mode of operation. Too passive, according to the authors of the report. "Once granted its annual budget," they pointed out, "NSF has to a large extent followed a practice of waiting for talented outsiders to suggest . . . projects on which to spend it." The Daddario subcommittee wanted this practice changed. "Foundation guidance in focussing upon . . . areas of appropriate research and education," its report argued, "could be a major factor in maintaining the stability of a civilization . . . threatened by the surfeit and concentration of people and their problems."

A third complaint from the subcommittee was that NSF's support of the social sciences had been inadequate. Here was an old issue—one that had risen repeatedly in the late 1940's, during the long legislative wrangle preceding the issuance of the first NSF charter. Should the

Foundation limit its benefices to the so-called "hard" or natural sciences—or should it bestow them also on the social sciences?

In 1945, at the start of the first congressional hearings to deal with the legislation that 5 years later would produce the original enabling act, Dr. Isaiah Bowman had put a sharp focus on that question. The Johns Hopkins President asserted that the study of social phenomena entailed "so much of human prejudice . . . and social philosophy" that the "widest divergence of opinion" existed as to when such pursuits constituted research, in the scientific sense of the term, and when they didn't.[6] Testimony of other witnesses at those early NSF hearings and a national survey indicated that most of the country's natural scientists thought that the labors of their sociological brethern "should have an integral place in the program." A contrary view, however, came from powerful Senators, including some of the strongest advocates of the proposed Foundation. Those were the Cold War days, and behind the Senators' wish to confine Government support to the natural sciences lay an awareness that many Americans regarded most social investigations, not as searches for objective truth, but as politically inspired schemes to reform society.[7]

In *Science, the Endless Frontier,* Vannevar Bush had taken it for granted that the research foundation envisioned by that landmark document would confine its concerns to "the natural sciences, including biology and medicine."[8] Nevertheless it was Bush who in late 1946 resolved the issue by endorsing a suggestion, previously advanced by some of the Senators, that the authority of the Foundation to assist the social sciences be "permissive but not mandatory."[9] When in 1950 the first charter finally materialized, it reflected this compromise of the matter by directing NSF "to . . . support basic scientific research . . . in the mathematical, physical, medical, biological, engineering, and other sciences. . . ."

Under the phrase, "other sciences," the Foundation authorities could push the social sciences as slowly or as rapidly as they wished. They elected to move slowly. They began in 1953 by having Harry Alpert, a sociologist who had joined the staff that year, prepare a review of the field. Alpert found the social sciences to be a continuum, ranging from projects involving accepted scientific methodology at one end to those of a primarily speculative or philosophical nature at the other. He suggested that "for the time being" NSF limit its grants to the scientific end of the spectrum.

Alpert's advice became Foundation policy. For several years NSF support of the field was a backdoor operation, with the agency's funding of the social sciences taking place under the aegis of its natural science directorates and almost always at those points along the continuum where the natural and social sciences converge. In its Division of Mathematical, Physical and Engineering Sciences, for example, the Foundation installed a program covering projects in human geography, economic engineering, and statistical design. Through the Biological and Medical Sciences Division it underwrote work in anthropology and in several related sciences such as human ecology, demography, and experimental and quantitative social psychology.[10]

In 1958 the Foundation consolidated its project-support programs in the field by assigning all of them to a separate office, a unit that in 1959 became a division. The budget for the office was $750,000. Two years later the division was receiving over $2 million and by 1965 its budget had reached $10 million. In the same period, from the late 1950's into the early 1960's, the Foundation gradually expanded the field by extending its coverage in the social sciences to its fellowship programs.

In this manner, over the years, the Foundation had fostered a fairly wide swath of the social sciences. Not wide enough, however, according to the Daddario subcommittee. On the ground that "a better understanding of human behavior" stood high among the country's needs, that body proposed to rewrite the charter so as to include the social sciences among the named disciplines that NSF is obligated to promote.

The fourth aspect of the Foundation's activities to displease the Daddario subcommittee was the role that the agency had played in the development of national science policy.

In an extended discussion of this matter, the authors of *The National Science Foundation: Its Present and Future* went back to the promulgation by John F. Kennedy of Reorganization Plan No. 2 of 1962. Before this executive directive took effect on June 8 of that year, the White House science apparatus had consisted of the President's Science Adviser, a parttime panel of experts called the President's Science Advisory Committee or PSAC, and the inter-agency body known as the Federal Council for Science and Technology. Reorganization Plan No. 2 of 1962 added the Office of Science and Technology, simultaneously relieving NSF of its evaluating function and most of its policymaking functions by transferring them to this new and fulltime White House unit.

The members of the Daddario subcommittee did not suggest that any of these chores be returned to the Foundation. On the contrary they agreed with observers who argued that it was unrealistic to expect one Federal agency among many to tell the others what to do or to bear the whole burden of science-policy formation. What the subcommittee did suggest was that NSF make a greater effort to work with the other agencies—and with Congress—in the pursuit of such policy. It seemed to the lawmakers that in recent years NSF had shown an inclination to hold aloof from the other elements of the Government as though fearful that association with them might detract from its position as the keeper of the sacred flame of pure research.

With this tendency the Daddario group had no patience. Never mind that on occasion cooperation with the other agencies and with Congress might involve the Foundation in political situations. In the view of the lawmakers NSF was now old enough and big enough to come out of its ivory tower and function in the real world.

"There should be," the subcommittee report pointed out, "and is, a scientific and technological stature about the Foundation sufficient to warrant an extraordinary voice in the science policy chambers of the administration. NSF is the only Federal agency with an exclusive scientific mandate. It should make itself heard, and should be listened to accordingly. . . ." To which the framers of the subcommittee report added that

> . . . .In relation to the Office of Science and Technology and the President's Science Advisory Committee, the Foundation should take the initiative and be held broadly responsible for the Nation's science resources, disengaging OST and PSAC from their detailed oversight in this area. OST and PSAC would thus be enabled to devote more time to national issues of applied science and development and to marshaling the resources of science and technology toward security problems such as coping with certain international difficulties. . . .[11]

Closely related to these views was the subcommittee's insistence that ways be found to give the National Science Board more visibility as a *national* advisory body, "available to both executive and legislative councils, with particular reference to science resources and policies governing them."[12]

So much for what Daddario called the "philosophy and rationale" behind the framing of the second charter. Its concrete stipulations, over and above those already mentioned, can be best viewed in connection with the legislative process that brought them to life.

The first version of the Daddario bill—H.R. 13696—was introduced in the House on March 16, 1966. Although its essential provisions were not unlike those of subsequent versions, it would appear that Daddario did not expect H.R. 13696 to go to a vote. It was a trial-flight bill, designed to stimulate discussion and sound out interested opinion.

Statements made during brief hearings in the early spring can be taken as a measure of the reluctance with which spokesmen for the scientific community brought themselves to accept the inclusion of applied research in the agenda of the Foundation. All of the key witnesses, including NSF Director Haworth, approved of the idea "on balance." Several of them, however, made no effort to hide their apprehensions about it.

Speaking for the Administration in his capacity as Executive Assistant Director of the Bureau of the Budget, Bill Carey called the issue "a close question." The President of the National Academy, Dr. Seitz, voiced concern "that the support of applied research at academic institutions may be the thin edge of the wedge which could ultimately result in pressures to support mission-oriented work at the expense of basic research." Seitz saw this as sure to exert "a damaging effect upon science, upon our academic institutions and, in the long run, upon our economic well being. . . ." The doubts of the National Science Board Chairman, Eric A. Walker, came out in an exchange with the subcommittee chairman. Told by Daddario that he "ought not to be so fearful about the fact that this word 'applied' begins to creep into this legislation," Dr. Walker replied:

> But there are so many big areas in which we need a great deal of research that . . . could inflate the request of the National Science Foundation to the point where I think we would horrify the Congress as well as ourselves.
>
> Let me give you one example. There is a lot of talk now about solid waste disposal. How do we get rid of junk cars? There is very little being done on this problem because there is no profit in it. . . .Somebody has to do some real applied research on developing machines and systems to get rid of discarded things. This is not going to cost $25,000 or $50,000. . . . someone may come in with a request for a hundred million dollars to do applied research on just getting rid of junk cars. This could be a large item in the NSF budget.[13]

On May 3, Daddario replaced H.R. 13696 with a mildly revised bill. On July 18 this measure, H.R. 14838, having been slightly amended on the floor, received the approval of the House and went to the other branch of Congress. There it was referred to the Committee on Labor and Public Welfare, chaired by Democratic Senator Lister Hill of Alabama.

In September Director Haworth drafted a letter to Senator Hill, endorsing the overall intentions of H.R. 14838, but requesting changes in some of its provisions. Concerning what the NSF head called the "most dramatic" of these—the proposal to permit the agency to fund applied research—Haworth wrote that:

We (the Foundation) believe this legislative mandate desirable because—

1. Limitation of the programs of the Foundation to "basic research" is an unclear mandate since the line which divides basic and applied research is frequently unclear, is certainly arbitrary, and often reflects the attitude of the investigator, his organization, or the agency which supplied supporting funds, rather than the intrinsic attributes of the research.
2. By restricting its programs to "basic" research, the National Science Foundation may be causing some scientists, and particularly some engineers, to direct their research activities and, hence, their educational impact also, away from their legitimate applied interests.
3. There is a genuine need for support, at academic institutions, of applied work which will help to bridge the gap between fundamental findings and socially useful systems and concepts.
4. There are many large and serious problems affecting society, which do not lie clearly within the mission of any other Federal agency, for which the Nation acutely needs new technological solutions.

But in the fall of 1966, the powerful Bureau of the Budget was not yet ready to go as far as the NSF Director. A handwritten note on Haworth's draft letter to Hill shows that it was never sent, having been marked "disapproved" by the Bureau.[14]

Strictly speaking then, NSF had as yet no official position on the effort to amend its enabling act when at the close of the 89th Congress in December, H.R. 14838 disappeared owing to the failure of the Senate to act on it. As a result of this development, most of the significant phases of the legislative process took place following NSF's receipt in January 1967 of word that Daddario had readied a new version of his bill. It was this measure, introduced on February 15 as H.R. 5404, that, as amended by the Senate, would become the second NSF charter.

* * * * *

What were some of the more drastic alterations in the nature and role of the Foundation contemplated by the bill as it stood in early 1967?

Whereas the clause that authorized support of applied research was permissive, other sections of H.R. 5404 commanded the agency to shoulder new tasks. Throughout its history NSF had been obliged to collect and collate data on the country's scientific and technical resources. The Daddario bill gave the Foundation the additional job of analyzing and interpreting such data. Another section of the measure greatly strengthened the agency's authority to participate in international scientific enterprises. Still another section extended the work involved in getting the Foundation's annual fund by requiring the agency to obtain authorization for its projected programs from appropriate oversight committees in both branches of Congress before submitting its budgetary requests to the House and Senate appropriations committees.

Several provisions of H.R. 5404 had to do with the administration of the agency. One was intended to improve the status of the Director by lifting him on the Federal pay scale from Class III to Class II, thus putting him on a par with most of the heads of other independent Federal agencies. Another, soon to foment a little controversy, created five new Presidential posts: Deputy Director and four Assistant Directors, all of them to be appointed by the President with the advice and consent of the Senate.

Still other clauses were aimed at ending the old duality of power between Director and Board. This was to be accomplished by vesting in the Director practically all of the authority relating to the management and operation of the Foundation. No longer, for example, would the NSF chief have to ask the approval of the National Science Board for grants and contracts, except when these disbursements signaled the start of a new program or the sums involved came to at least $500,000 a year or $2 million in all, figures that the Board could reduce if it saw fit to do so.

Just as some portions of the measure clarified the authority of the Director, so others did the same for the Board by freeing that body from all operational duties and largely limiting its responsibilities to the establishment and supervision of the Foundation's internal policies. The intent of others was to encourage the Board to play a more vigorous part in the promotion of external (that is, national) policies covering the promotion of basic research and scientific education. Once the bill became law the Board would be required for the first time to render annual reports to Congress dealing with "the status and health" of selected aspects of science and science training.

The closing weeks of January 1967 found Director Haworth conferring off and on with Carey of the Budget Bureau and Dr. Donald F. Hornig, science advisor to the President and Director of the Office of Science and Technology. These were strategy sessions. All three men agreed that sections of the Daddario bill ought to be altered; but should they try to achieve the revisions they wanted by submitting a separate administration bill or by simply asking for amendments in the measure as it stood?

A major outgrowth of their discussions took the form of a letter from Carey to Haworth, suggesting that the Administration forces follow the amendment route. As for the plan to permit the funding of applied research, Carey proposed that they defuse that issue by "creating strong legislative history"—which is to say by testimony before the Congress stressing the determination of the Foundation to avoid any use of its new power likely to impair the agency's traditional emphasis on basic science.[15]

Calculatedly or otherwise the NSF leaders and their friends had been following this procedure all along. At the time of the Daddario subcommittee's first public hearings in 1965, no legislation had as yet appeared. Most of the witnesses, however, were aware that a provision allowing the funding of applied science was under consideration. None of them opposed the idea, but almost every scientist to take the stand, accompanied his pro forma endorsement of it by an extended and often fervent exposition of the values of basic research and of the demonstrated ability of the Foundation to support it.

Not untypical in this respect were the remarks of Don Price of Harvard. "I should think," Price told the subcommittee, "that the National Science Foundation would do well to concentrate in the main on its original purpose of support for basic research and related educational activities and not get pulled too deeply into programs of applied research." It seemed to the Harvard dean that "only as long as the Foundation concentrates on basic research will it in a sense keep the great mission-oriented agencies on their toes in their support of basic research by setting high standards of quality and competence. We build a lot of free competitive enterprise into our government structure, and I think this is a very good thing indeed and one of our main safeguards."

Price's words brought a question from Chairman Miller of the full Committee. "Isn't it true," Miller asked, ". . . that there are programs such as weather modification and oceanography that do not find themselves within any one department of Government?" Price conceded that under special circumstances the Foundation should be called on to support programs involving applied research. He repeated his conviction, however, that the extent to which such programs were funded should be governed by the line from Gilbert and Sullivan: "What never? Well, hardly ever!"[16]

Similar sentiments sounded from the witness stand during the second public hearings in 1966, and the Gilbert and Sullivan spirit still prevailed at the Foundation and in much of the scientific community when on April 12, 1967, H.R. 5404 was voted by the House, and the locus of the action shifted to the United States Senate.

Several weeks before House passage of the bill, NSF had requested a number of changes in it.[17] The Congressmen had ignored these pleas, but the transfer of the measure to the Senate gave the Foundation another chance to ask for amendments. In a letter to Lister Hill, as chairman of the Senate Legislative Committee, the Foundation reiterated its disapproval of portions of the House bill and expressed the hope that the Senate would alter it accordingly. Dated April 24, 1967, and signed by Director Haworth and Board Chairman Philip Handler, the NSF communication cited five provisions as either wholly or partly unsatisfactory.[18]

One was a directive under which the agency would be obliged to keep track of the distribution of all Federal funds for science and technology. Once a year, under this prescription, the Foundation would have to tell both the President and the Congress what Federal bureaus had given what sums for those purposes to what organizations.

Speaking for the Foundation, Haworth and Handler said that they were not averse to "this provision insofar as it relates to educational institutions." They had "serious reservations," however, "about the feasibility of fulfilling this requirement" for nonacademic entities, including industrial firms. "In particular," they observed, "it would be difficult to ascertain the costs of research conducted under contract provisions that do not break down the contract price in terms of various elements. Furthermore, the identification of research expenditures made by subcontractors would in many cases offer insurmountable difficulties." They suggested, therefore, that the bill be revised "to indicate that nonacademic organizations need be included only to the extent deemed feasible by the Foundation."

Another of the Foundation's recommendations was that a clause of the Daddario bill requiring the Board to foster the pursuit of national science policy be rewritten so as to give the same privilege to the Director. "Our concern," Haworth and Handler wrote, "is that this Section as now phrased could be interpreted as limiting one of the proper functions of an agency head, viz., to recommend policies and actions in which the agency has a vital concern."

Still another request was that a section permitting the Board to set up a permanent staff be dropped. Under the enabling act as it then stood, the Board relied for services of this sort on the regular staff of the Foundation. According to Haworth and Handler, that arrangement had proved satisfactory and they saw no reason to change it.

Of the two remaining requests, one was that no dollar limit be placed on the right of the Director to issue grants and contracts without Board approval. The other one urged elimination of the requirement that the four new Assistant Directors provided for in the

House bill be named by the President. The Foundation leaders' feeling about this stipulation was that it would give NSF "an excessive number of Presidential appointees for an agency of its size, and would remove virtually all the top positions of the Foundation from career status."

Formal Senate action began on October 30 with the introduction by Democratic Senator Edward M. Kennedy of Massachusetts of a companion bill numbered S. 2598 that reflected all of the Foundation's suggested changes. Hearings conducted in mid-November by a subcommittee chaired by Kennedy gave interested parties an opportunity to discuss the amendments set forth in the Senate measure.

Appearing as a witness, Daddario directed most of his fire at the proposal to delete the requirement that NSF keep tabs on money going to nonacademic contractors. "In light of our experience with the space program," the Connecticut Congressman said, "we saw develop in NASA a capability to keep track of Government funds [for science] going to the private contractors down to the second and third tiers, and we found it both useful and possible that these funds be followed." Daddario granted that the work would be hard, but argued that NSF should not be excused from doing it for that reason. "If we do not do it at a time when it is difficult," he said, "as time goes on it will . . . become impossible."[19]

Daddario lost that battle. The Senate went along with NSF's request for the deletion. It also honored the Foundation's recommendation that specific authority to participate in the development of national science policy be given to the Director as well as to the Board. Otherwise the bill, as passed by the Senate on May 24, 1968, was identical in all important respects with H.R. 5404 as originally drafted by Daddario.

House acceptance of the Senate amendment had the effect of accelerating the legislative process. On July 8 the bill went to the White House. Ten days later it became law—and the National Science Foundation would never be quite the same again.

Chapter 12. Revising the Charter

[1] *The National Science Foundation: Its Present and Future,* Report of the [House] Committee on Science and Astronautics, 89th Cong., 2d Sess., Washington, 1966, p. xiii (cited hereafter as Present and Future).

[2] Personal interview.

[3] *National Science Policy. H. Cong. Res. 666.* Hearings before the Subcommittee on Science, Research, and Development of the [House] Committee on Science and Astronautics, 91st Cong., 2d Sess., Washington, 1970, p. 4.

[4] Review Hearings I, p. 2.

[5] Present and Future, p. xi.

[6] *Hearings on Science Legislation (S. 1297 and Related Bills).* Hearings before a Subcommittee of the [Senate] Committee on Military Affairs, 79th Cong., 1st Sess., p. 23.

[7] Gene M. Lyons, *The Uneasy Partnership: Social Science and the Federal Government in the Twentieth Century,* New York, 1969, p. 133.

[8] Ibid., p. 129.

[9] Ibid., p. 134; Harry Alpert, "The Role of the Foundation with Respect to Social Science Research," National Science Foundation Staff Papers: A Report to the Director, April 15, 1954, Appendix A.

[10] Lyons, ibid., p. 272.

[11] Present and Future, pp. xv, xvi.

[12] Ibid., p. xvi.

[13] *Amending the National Science Foundation Act of 1950. . . .* Report to accompany H.R. 5404, 90th Cong., 1st Sess., Washington, 1967, p. 14.

[14] Draft letter, Sept. 30, 1966, OGC Files.

[15] Jan. 20, 1967, ibid.

[16] Review Hearings I, p. 252.

[17] Philip Handler and LJH to Miller, Feb. 20, 1967, OGC files.

[18] In OGC files.

[19] *National Science Foundation Act Amendments of 1968.* Hearings before the Special Subcommittee on Science of the [Senate] Committee on Labor and Public Welfare, 90th Cong., 1st Sess., Washington, 1968, p. 112.

Chapter 13

New Faces and New Directions

For the bosses of the National Science Foundation the enactment of the second charter in the summer of 1968 could scarcely have come at a less opportune moment. Haworth's 6-year term as Director was about to end, and the word following the national elections in the fall was that the incoming President, Richard M. Nixon, intended to replace him. Under these conditions Haworth refrained from filling the new high-level posts created by Congress on the grounds that his successor would want a free hand in this regard.

As for the agency's newly broadened authority to support applied research—here, too, the Director and his aides were at a disadvantage. A change of such magnitude called for a substantial reorientation in their thinking—a leap of the mind difficult, if not impossible, for what now amounted to a "lame duck" leadership. Add to this handicap the ingrained attitudes of an organization dedicated from its beginning days to pure science, not to mention the fear of change built into the bureaucratic structure.

Given these circumstances it is hardly surprising to find that NSF's early efforts to implement its applied science authority were marked by a false start and some faulty tactics. Haworth's first move consisted of drafting an "Important Notice" for distribution to the heads of universities and other eligible research centers. Labeled "Support of Applied Research" and approved by the National Science Board, the Notice announced that "the Foundation is now ready to consider proposals for scientific research whether basic or applied. . . ."

It is NSF's practice to make known the availability of funds for new kinds of endeavor in this fashion. In this instance, however, the Notice never reached its intended readers. It got no farther than the Hill where it withered before the stern disapproval of Congressman Daddario.[1]

Daddario's major objection was that the Notice represented an inaccurate reading of the law. "It was never our thought," he informed the Director in December 1968, "that the Foundation generally solicit

. . . research proposals of an applied nature in all fields of science. Such an interpretation would appear to reduce the total support of basic research . . . simply in order to be doing some applied research. This would be a distortion of our intent from permission for applied research into a mandate to support such work." What the framers of the law contemplated, according to Daddario, was that NSF confine its promotion of applied research to "the solution of major national problems," the "furthering of engineering research into early phases of operation" (with the idea of turning such activity over to "other groups" for development), and the identification of whatever "neglected facets of modern research" the Foundation considered to be in need of attention.

"It would be unfortunate indeed," he warned, "if in this period of severe funding limitation, the science community were to gain the impression that the Congress . . . wished to dilute basic research from the Foundation." As the Connecticut statesman saw it, the "key" to understanding the new charter was "*permissive authority* to support *relevant* applied research."[2]

In retrospect Daddario's complaints about the Important Notice seem to have been a little over subtle. Subsequent to the enactment of his bill, a number of academic scientists had strongly urged him to limit the Foundation's support of applied science, and one former NSF official has suggested that by December 1968 Daddario may have been having some "second thoughts" on the subject. His law clearly authorized the Foundation to consider proposals for work in applied science, and it was incumbent upon the agency to so notify the scientific community. Nonetheless Haworth bowed to the Congressman's wishes to the extent of not issuing the Important Notice. He did see to it that the NSF publication covering the rules for the submission of requests for grants was changed to reflect the new situation.[3]

By the time Daddario's objections to the Notice reached Haworth, the Foundation had already begun work on a different approach to the problem.

Even before the passage of the Daddario bill the question of how applied research should be brought into the picture at NSF had been a topic of considerable discussion among members of the staff and at sessions of some of the agency's advisory panels. On October 15 the Advisory Committee for Engineering embodied its views in a formal resolution. Three propositions dominated this proposal, much of which was the work of Dr. William G. Shepherd, an electrical engineer and a Vice President at the University of Minnesota.[4] One was that the Foundation as a whole, rather than any one element of it, support "programs in both education and research dealing with the application of interdisciplinary [academic] faculty talents . . . to the problems of contemporary life." Another was that such undertakings be funded by "block grants to institutions, or consortia, that submit acceptable proposals and . . . demonstrate the requisite faculty capability. . . ." Manifestly aimed at avoiding the short-term investigations common to the mission agencies, a third stipulation was that support for any accepted project be "at a level sufficient to sustain" its participants "for a continuous time period of a reasonable length."[5]

Haworth accepted these recommendations in principle, and then altered them in detail. Instead of the Foundationwide activity outlined by the engineering group, he called for the creation of a separately managed and budgeted program for the support of problem-oriented research, applied or otherwise. Having limned his ideas in a brief paragraph, he gave the job of fleshing them out to Dr. Randal M. Robertson, who as Associate Director for Research headed the Directorate where the new program would be lodged.

Haworth's choice reflected his awareness that Robertson, a physicist with a doctorate from MIT, had spent most of his working life in industry and in other nonacademic organizations. Of his almost 13 years with the Office of Naval Research, Robertson has written that "ONR was characterized by a philosophy, which I helped establish, of making sure that the Navy derived maximum benefit from science. To do this we supported basic research in areas of long range relevance to Navy problems, applied research directed toward Navy requirements, and exploratory development aimed at demonstrating the feasibility of radical new systems and operational procedures. This latter activity was aimed in part at showing the relatively conservative bureaus of the Navy some new tricks."[6]

Coming to NSF in 1958 as Assistant Director for Mathematical, Physical, and Engineering Sciences, Robertson brought with him "a major interest in applied science." It is axiomatic among scientists that what can be called basic research in one field may be just as legitimately viewed as applied research in another; and that in all fields the line between the two is sometimes a shifting and shadowy one. Both in his first post at NSF and later as head of the Research Directorate, Robertson urged his program directors to define basic research as broadly as possible even if this meant funding some projects obviously in the gray area where basic leaves off and applied begins. He has described himself as chafing under the legal restraints of NSF's first charter and as happy to see the agency's mission broadened by the Daddario bill.

His first step, when asked to set up NSF's applied-science program, was to assemble a task force, drawn chiefly from the engineering and social science sections of the agency, to assist him in the preparation of a plan for consideration by Congress.

The task force's first name for the project program was Interdisciplinary Research on Problems of Society or IRPOS. By the end of 1968, however, it had been changed at the urging of the Director to Interdisciplinary Research Relevant to Problems of Our Society or IRRPOS.* Haworth thought the original title promised too much. He preferred IRRPOS largely because it did not suggest that the Foundation was going to heal all of the ills of society overnight.

It has been written of Haworth that he saw NSF's new power to support applied research as first of all a means of encouraging more interdisciplinary scientific activity in the colleges and universities and only secondarily as an opportunity to tackle social problems.[7] It is true that IRRPOS, as formulated by Robertson and his task force, stressed the importance of breaking down disciplinary barriers in the country's academic institutions, where one often gets the feeling that biologists speak only to biologists and physicists only to Einstein. It is also true that there was discontent with this emphasis on the part of both the legislative and executive branches of the Government—and that this situation would be instrumental in the disappearance of IRRPOS only a couple of years after it got started.

In a letter defending the ideas that went into IRRPOS, Robertson points out that NSF's role in applied research must be viewed in the context of the Federal science establishment as a whole. "For me," he writes, "the so-called mission agencies must be the key to most problem-oriented research. You merely have to think about the possibility of NSF having a major responsibility for research on submarine warfare or nuclear weapons or agriculture or cancer to recognize the importance of limiting [its] . . . role and assigning problem solving to agencies set up and staffed for the purpose. . .[NSF's] problem solving should aim at high risk innovation and at showing the way in selected areas to the more conservative mission agencies. It would be a mistake, in my opinion, for NSF itself to become a mission agency, with final responsibility for some sector of the practical world. . . . I see NSF as having an absolutely indispensable central role in the total enterprise, but a definitely limited one. . . ."[8]

Early in 1969 IRRPOS became a line item in NSF's budget request for fiscal year 1970. Haworth had originally considered $15 million a suitable starting fund, but the request approved by the Bureau of the Budget was $10 million.

---

* In many official papers, NSF continued to use the shorter acronym, IRPOS.

Dr. Robertson's task force, meanwhile, had drafted a 4,500-word paper for delivery to Congress—a document that later on would serve as the principal planning guide for the actual creation of IRRPOS. Many of its ideas were from Dr. Joel A. Snow, a 32-year-old newcomer to NSF who during his recent days as a graduate student at Washington University in St. Louis had served as a volunteer with a local group interested in the application of scientific knowledge to environmental problems, later moving on to do somewhat similar work in the Interdisciplinary Research Center at the University of Illinois. The National Science Board had endorsed the paper. In truth it had done so with surprising enthusiasm in view of the propensity of many of its members, including Chairman Handler, to flinch at the prospect of putting into practical research money they would rather have seen bestowed on investigations of a purer nature.

In the early spring the paper made its way to Congress, and on the morning of Tuesday, March 26, Robertson went up the Hill to explain its contents to the members of the Daddario subcommittee.

The unveiling of IRRPOS in Congress took place under circumstances unprecedented in the history of the National Science Foundation. For 18 years the process by which the agency acquired its annual funds was a relatively simple exercise. Once NSF's estimates received the blessing of the budget bureau, its managers had only to justify them in appearances before the House and Senate appropriations committees. A clause in the amended enabling act of 1968 lengthened and complicated the procedure by requiring the Foundation, before offering its budget requests to the funding units, to obtain authorization for them from NSF's congressional overseers, Daddario's subcommittee in the House and the special subcommittee for NSF chaired by Edward M. Kennedy in the Senate.

It was during the Foundation's initial authorization hearings, then, that Robertson undertook to justify the plans for IRRPOS. He was notably nervous for several reasons. Frequently, during the preceding months, he had requested permission from Haworth to call on Daddario for the purpose of explaining the new program to him and of getting the subcommittee chairman's advice and guidance. In what was plainly a tactical error, Haworth had failed to act on these pleas. He thus made it impossible for Robertson to confer with Daddario in private since it was firm NSF policy that "all significant contacts" with Congressional committee chairmen must be made through the Director. Consequently, Daddario had learned about the IRRPOS plans through formal budget channels. As the father of NSF's involvement in applied research he was "predictably angered," to quote Robertson, "that he had not been consulted."

In addition, Robertson was acutely aware that IRRPOS was an experimental program. Its progenitors could only learn how to do it by doing it. In Robertson's opinion the plans drafted by his task force and especially those framed by Joel Snow were "splendid." Nevertheless they featured many untested suggestions for procedure. If Daddario chose to challenge any of these assumptions, he would find it "child's play" to do so.[9]

The drama that took place in the committee room that Tuesday morning bore out Robertson's apprehensions. In charge of presenting the numerous activities of the Research Directorate, he had hoped to introduce IRRPOS with a few general statements, leaving the airing of the details to a later point in his testimony.

He was not able to proceed in this manner. He had bestowed precisely 107 words on the new program when Daddario interjected the first of what was to be a string of sharp questions and blunt comments. The subcommittee chairman observed that as a starter IRRPOS intended to "make 40 exploratory planning grants and 16 large ones." He wanted to know how Robertson and his task force had arrived at those numbers. "What judgment values did you feed into this process to arrive at the 56 figure?" he asked. "What do you expect to gain from it?" During the morning he would throw out that question over and over, in various forms: "What do you expect to accomplish through it? What is it going to give us that we do not presently have?" Having voiced his first version of it, Daddario asked Robertson if he had "any idea" as to what sort of projects IRRPOS was planning to underwrite?

Robertson tried hard to placate the obviously angry chairman. He said the figures he had cited were "based on discussions with people interested in doing this kind of work." As to the sort of work IRRPOS would support, that, naturally, would deal with societal problems. What problems, the chairman demanded. Robertson mentioned "air pollution" as one of many possibilities. Daddario then wanted to know what procedure IRRPOS meant to set up for the purpose of singling out the problems to which the country's scientists should address themselves.

No difficulty there, according to Robertson. IRRPOS would simply ask colleges, universities, and other nonprofit research centers to submit suggestions in the form of proposals. He hastened to add that the Foundation was in "a very excellent position" to do this "because of our close relationship with and knowledge of the academic science community in this country."

Daddario pounced hard on that statement. "Dr. Robertson," he said,

> maybe that is the problem. You are approaching it [the establishment of IRRPOS] as you have approached your whole basic research situation. You have this capability. You have dealt with the universities. You are following that same program. It may not be the formula that is necessary at this time, and you may be doing yourself harm and the universities harm by not looking to a new mechanism, through which certain criteria or knowledge can be developed on which the universities can then develop support.
>
> I think they are going to be reaching out in a less than emphatic way. They are groping; you are groping; and you are kind of groping together. I don't know that that is going to come up with something substantial enough in the applied area for the Congress to support. . . .

The exchange went on all morning with Daddario, as the noon adjournment neared, pointedly reminding Robertson of an old saying: "Do not confuse the importance of the problem with the quality of the solution."[10]

Obviously the House subcommittee was not yet convinced that the NSF leadership had come up with a viable solution to the question of how NSF should apply scientific knowledge to pressing national problems. When a short time later the 1970 budget was voted, it showed a reduction in Haworth's request for IRRPOS from $10 million to $6 million. Such was the status of NSF's first effort to utilize its expanded applied research powers as Haworth made ready to leave the Directorship on June 30 and the process of finding a replacement for him moved into its final stages.

It turned out to be a troubled process. As always the National Science Board had started it by suggesting suitable candidates for presidential consideration. Early in 1969 NSB sent to the White House the names of two eminent physicists, Emanuel R. Piore, a vice president of International Business Machines Corporation, and H. Guyford Stever, president of Carnegie-Mellon University in Pittsburgh, Pa. At first both men indicated an interest in the position, only to withdraw later with Stever explaining that his university was in the midst of a fund drive and other activities he himself had initiated and wished to see through.

The Board began again. Two more names went to the White House, and by the end of March Lee DuBridge, Nixon's science advisor, had endorsed Franklin A. Long of Cornell University and Long had agreed to take the job. End of the search for a new Director? Not quite.

An effort by DuBridge to bring Long and the President together fell through when Nixon had to cancel a scheduled meeting to attend the funeral of the late President Eisenhower. A short time later new arrangements were made. Under these plans Long was to see Nixon at the White House on the afternoon of Friday, April 11.

At the time of Long's arrival in Washington on the morning of that day all seemed to be in order. The plans were for Long to call first on DuBridge, and it was assumed that after his talk with the President, tentatively set for 3 p.m., the announcement of his appointment would be made.

But these expectations were not realized. On reaching DuBridge's office Long learned, as he later told the press, that "the situation had changed" due to "new elements of a political nature."

The "new elements" revolved around the efforts of the Nixon administration at that time to win congressional approval for an antiballistic missile system to which the Cornell scientist, a liberal Democrat and a veteran battler for arms control, was known to be opposed. As would come to light later, influential legislative supporters of the ABM had concluded that the appointment of Long to a high Federal position would spread the impression in Congress that the Administration's advocacy of the system was less than firm. Yielding to this congressional reasoning, members of the White House staff had taken the stand that the Cornell scientist's name should not be submitted to the President and Nixon had gone along with them.

According to information assembled by *Science* magazine, the morning conference between DuBridge and Long was a prickly encounter. DuBridge said that Long "could have the NSF post" if he would agree either to support the antiballistic missile system or to let Nixon withhold the NSF appointment until such time as Congress had voted the system up or down. Were neither of these arrangements satisfactory to Long, DuBridge was quoted as asking the Cornell scientist to withdraw his name, alleging some "personal reason" for doing so. Long pronounced all three alternatives "totally unacceptable" and hastened back to Cornell where biographies of him prepared for release following the announcement of his appointment were quietly returned to the files.[11]

On April 17 the news of what had happened and why was on the front page of the *New York Times.* On the 18th the board of the Federation of American Societies for Experimental Biology, during an annual meeting attended by 22,500 members in Atlantic City, issued a statement saying that "political considerations should not enter into the appointment of a National Science Foundation director." Simultaneously similar contentions began coming from NSF's friends in Congress, Representative Daddario and Senator Kennedy among them. On the 21st the National Science Board, in what *Science* called an "audacious action for a governmental body," produced an open letter deploring the Administration's break with the long-honored practice of keeping the affairs of the Foundation out of politics.

These and other protests were not without effect. On the 28th the President played host to members of the National Science Board and of the National Academy of Sciences. During a session marked by great cordiality he confessed that he had been wrong in his handling of the Long appointment and that he now agreed with his visitors' desire to bar politics in the filling of top NSF positions. The President also revealed that during the preceding week he personally had offered the Directorship to Long. The Cornell scientist had described himself as profoundly pleased by Nixon's request. He had turned it down, however, explaining that because "earlier events had made" him a politically marked and polarized figure his assumption of the Directorship "would make the operations of the National Science Foundation . . . difficult."[12]

Previously there had been a widespread fear, to quote Daddario, that as a result of the fiasco over Dr. Long "recruiting a director for the National Science Foundation of the desired competence and ability . . . will now be extremely difficult, if not impossible." Nixon's admission of error eliminated this concern, and the National Science Board returned to the search.

During the opening days of the selection process, Board Chairman Handler had asked the head of the biology department at Johns Hopkins University, biochemist William D. McElroy, if he would be interested in "having his name in the hat." Dr. McElroy's response was no, for which he advanced several reasons. One of them was that he was "still having fun in science" and had no desire to leave his laboratory for an administrative post. (His major scientific interest is bioluminescence, and his many contributions to the field include the discovery that the mechanism by which fireflies and other creatures make their own natural light is akin to the process by which all animals convert chemical energy into mechanical energy.) Another of his reasons for saying no to Handler in early 1969 was his wife. Marlene Andregg McElroy was happy in Baltimore.

When in late April, Nixon cleared the air by owning up to his mishandling of the Long case, Handler approached the Johns Hopkins biochemist again. McElroy's reconstruction of their talk is that "Phil and I were old friends, and he just phoned me one day and gave me the disaster story. He said the Science Foundation was going to hell, support of science was going to hell, and they had to have somebody at NSF who could do the job. Well, that's a hard one to turn down."

Although McElroy considered Handler's apocalyptic views somewhat overwrought, he realized that they were shared by much of the scientific community. "Put it this way," he says. "Somebody new at the White House could have selected someone arbitrarily, which could have been a disaster to the scientific community. Still that was a tough decision for me. For my wife, also. She had her spurs on the ground, holding on to Baltimore all the way, until we got to Washington and she discovered she could have a good time there too."[13]

Nixon had promised the Board that his next nominee would be chosen without reference to political considerations. Not that McElroy posed a problem in this regard. Although a registered Democrat who says he tends to vote as an independent, he was not on record where the ABM was concerned. On June 19 the President announced his nomination. On July 11 the Senate confirmed him, on the 14th he took the oath of office in the presence of the President's Science Advisory Committee, and on the 15th he began his new duties. He was 52 at this point, a tall and robust individual with a genial countenance, a balding pate, and the almost perpetual tan of a man given to golf, sailing, and the pursuit of scientific ends in the open areas of such places as Woods Hole, Mass., and the Caribbean.

He spent much of his first day as Director touring the offices of the Foundation, exchanging smiles and handshakes with the employees.

On the next day he went up to the Hill. The date—August 16, 1969—was an historic one. Down at Kennedy Space Center in Florida a million people had gathered to watch the Apollo 11 astronauts begin the 238,857-mile trip that would put the first human beings on the moon. The Senate Office Buildings, McElroy's destination, had a deserted air. Many of their occupants had gone south for the space shot. At least one longtime NSF-watcher, however, was on hand—Senator Gordon Allott.

Since his falling out with Waterman during the Project Mohole days the influential Colorado statesman had been consistently cool to the Foundation. When McElroy left his office after a long confab, it was with the satisfied feeling that henceforth Allott would be a useful friend to NSF.

Watchful members of the Foundation staff read into McElroy's opening gambits an indication of drastic changes to come, and a fashion note from one of them underscores their understanding of the new tone brought in by the new Director. "Under Waterman and Haworth," it runs, "the men here at NSF wore narrow ties and white shirts. After McElroy took over, they switched to wide ties and colored shirts with large collars."

It is unnecessary to add that the changes went deeper than that. How deep, it is a little too soon to say. McElroy's stay at the Foundation was brief, ending 2½ years after it began when he resigned to become

Chancellor of the University of California, San Diego. Early in his regime he had announced his intention of boosting NSF's annual appropriation to a billion dollars—a statement that one knowledgeable observer has described as "one of the most important he ever made" in that it served notice of his determination to create "a more progressive and dynamic agency." In point of fact the budget rose during his regime from $440 million when he took office to $650 million (for the coming fiscal year) when he resigned in 1972. That the Foundation had achieved a more exciting look during his Directorship is undeniable. The fact remains that at the time of his departure the major changes he had introduced were just beginning to take form, and at this writing must be regarded as still on trial.

The origins of the philosophy of expansion that McElroy brought to NSF are readily discernible in his background. Born and brought up on a farm near Rogers, Tex. (population about 800), he moved in his early teens to the Rio Grande Valley to attend the junior and senior high schools at McAllen, Tex. By graduation time he had long since lost his parents and the job of obtaining a higher education was up to him. He accomplished it by migrating to California where for $25 he could matriculate at one of the State-supported junior colleges. In 1935, having borrowed $125 from the McAllen Kiwanis Club ("I figured I'd need a little spending money, too," he says of this transaction), he enrolled in the junior college at Pasadena.

Fortunately for his future, young McElroy had brawn as well as brains. In 1937 a football scholarship took him to Stanford University where during his junior and senior years he distinguished himself in the Rose Bowl and elsewhere as a hard-tackling right end.

At Stanford he pursued a premedical course. As a graduate student at Reed College in Portland, Ore., he switched to biology, earning his Master's degree there in 1941 and then moving on to Princeton where he received his doctorate in 1943. Followed a period of war research under the medical branch of OSRD, after which he returned to Stanford for a year as a National Research Council fellow. Joining the faculty at Johns Hopkins in 1946, he moved up steadily, eventually becoming the chairman of his department and director of the McCollum-Pratt Institute.

It was not his practice at Johns Hopkins to confine himself to office and classroom. During the 1964 presidential election campaign he worked with the Scientists and Engineers for Johnson and Humphrey. The community beyond the gates of his university knew him as a member of the Baltimore school board and a trustee for two of the city's smaller colleges. The youngsters of the town knew him as "the firefly man" because of his standing offer for several years to pay them $25 (later $35) for every hundred insects they captured for use in his experiments.[14] His champagne and oyster parties were social highlights and just about everyone whose name stood for influence in the greater Baltimore area came to them.

"Virile" and "humorous" are words often applied to him by admiring friends. "Flexible" is another. Indeed one gathers that where almost any accurate statement about him is concerned the obverse can be just as confidently asserted. "Scratch Bill McElroy deep," one of his friends says, "and you find a frontier Texan, blunt-speaking, informal, earthy and masculine." He is also a polished scholar. He enjoys a drink, holds his liquor well, and is at home in the company of people who like himself "are fond of the corpulence of life." He is equally at home in the austere environment of the laboratory and the study.

Shortly after entering upon his duties at NSF he told a colleague at the University of Chicago that "shifting from the passive to the active role is the Foundation's fundamental problem."[15] What he had in mind is brought out by David E. Ryer, a Special Assistant to McElroy at NSF now Assistant Chancellor at San Diego.

Ryer says that by the time McElroy became Director he had been "around the science establishment" long enough to have "formed some judgments about the Foundation." One of them related to the agency's largely reactive way of operating and especially to its practice (to hark back to a complaint of the Daddario subcommittee) of letting "talented outsiders" shape its research-support programs. It was McElroy's understanding that in Congress and elsewhere these passive procedures had produced the impression that the Foundation was "a captive of the scientific community." He was of the opinion that ways should be found to correct this image, taking care in the process not to damage the quality of the research funded by the Foundation. He also believed that any effort by NSF to play a more active role in the promotion of American science would set well with those who controlled its budgets not only on the Hill but at the Office of Management and Budget as the Bureau of the Budget was redesignated, effective July 1, 1970, by President Nixon. As a result more money for the support of science "would be forthcoming."[16]

McElroy's efforts to translate his thoughts into deeds took various forms. In a move to open up better lines of communication between the Foundation and the nonscientific segments of the populace, he considerably enlarged an existing NSF activity called Public Understanding of Science. He initiated a program in the Education Directorate to create science curricula tailored to the interests and background of nonscience students. He encouraged the establishment of the Student-Originated Research Projects Program, under which young people received grants for projects that they themselves proposed for "improving the quality of life."

His call on Senator Allott was only the first of numerous excursions to the Hill to win friends for the Foundation. Speaking of McElroy's aggressive wooing of Congress, Ryer pictures the Director as altering his techniques to fit his targets. To those lawmakers aware of science as a cultural activity, he talked "deep fundamentals." With those who weren't he resorted to "used-car salesmanship."

McElroy himself, reminiscing today in his San Diego office, traces his determination to better NSF's relations with the Hill to his perception of the mood of the country when he became Director. "There was obviously a down trend in the funding of science," he says. "Also there was a little bit of . . . anti-intellectualism around, an undercurrent of distrust of science, generated perhaps by the success of its efforts. Obviously the time had come to stop trying to get support for it by saying that 'science is beautiful' and to start emphasizing its utility to society. Certainly the long range thing was to get the American people to understand the critical importance of supporting science and especially fundamental research. To reach that goal with any speed one had to start with the elected officials. That's basically what our schemes and stratagems were all about. Pretty simple."

In keeping with his desire to broaden the appeal of NSF he also sought to broaden the National Science Board by recommending the appointment to it of more individuals from industry, even trying from time to time to bring in members innocent of any connection with science. That these efforts were not invariably successful may have contributed to the development, during his regime, of a certain strain twixt himself and the Board.

One of the most striking aspects of the Foundation under Waterman and Haworth had been the close and constant cooperation of Director and NSB. During the McElroy era that spirit diminished noticeably.

Board members complained that McElroy made "end runs" around them, and the record does show him putting at least one new program into effect without bothering to solicit their approval as required by law.[17]

McElroy's side of this story, as he recalls it now, is that such were his relations with the powerful Office of Management and Budget that "on occasion," he had no choice but to "fix a decision without consulting the Board." Every now and then, he explains, OMB would offer to back an increase in the budget of the Foundation on condition that NSF effect a specified change in its programmatic structure. As the Director soon discovered, when these opportunities arose, he had to say yes to them "in about 5 seconds" or pass up the proffered increase. The reason for this, he recalls, is that the OMB as then constituted was "a mighty impatient and dictatorial body." He says he "always would call up the Chairman of the Board. . ., informing him of what I was going to do." But was it ever possible to give the full Board an opportunity to consider an important OMB proposal? "No way!" McElroy exclaims.

It may be added that he was known now and then to express impatience with what he called the "parochial attitudes" of NSB. He was heard to say that the major contributions of some members to the deliberations of that body consisted of gripes that the scientific disciplines they represented were not getting large enough slices of the Federal pie. It is his recollection now that out in the scientific community the biologists spent a lot of time worrying over the possibility that, as one of them, he might be "leaning backwards in favor of other areas of science."

Since McElroy insisted that the Foundation be more "activist," it is proper to ask if by this preachment he meant that it should actually define what work the beneficiaries of its research grants should do and how.

"Absolutely not," says Dave Ryer. According to Ryer McElroy was as averse as the earlier NSF heads to the idea of "bureaucrats telling scientists what to do." What he did demand of his program directors—the staff members responsible for processing proposals from would-be grantees—was that they put more effort into locating the frontiers of scientific endeavor in their fields and of making sure that "the best men" working along those frontiers were supported.

There seems to be no evidence that either of his predecessors ever seriously questioned the judgment of their program directors as to what proposals should be funded. McElroy often did so. It was his impression that some of his program directors had become more bureaucrats than scientists, that they had lost touch with the branches of research over which they presided. On his travels to colleges and universities he made inquiries in an attempt to find out what the best young people in each field were doing and where they were doing it. Ryer pictures him, on his return to NSF, telling his program directors what he had learned and occasionally saying to one of them, "Now show me some good reason why you are not putting more money into the areas I've mentioned." He urged the program directors to spend more time visiting academic laboratories and subjecting themselves to the questions and complaints of working scientists. He stressed the need for larger travel budgets for these purposes.

Ryer describes McElroy as a "populist" in his eagerness to enlarge NSF's contacts with Congress and with the nonscientific public, but as "elitist" when it came to how the Foundation's research-support funds should be distributed. He believed that the cutting edge of scientific progress was almost exclusively the work of the most gifted "2 to 3 percent" of the people in the profession. He appreciated the value of the data collected by run-of-the mill scientists, by "the rock-squeezers who put rocks in ovens and try them at every temperature;" but when budgets were restricted, he took the attitude—to quote Ryer—that "we must support the very best even if it means wiping out the middle grid."

No sooner was McElroy on the job as Director in August 1969 than he began reshaping upper echelons of the Foundation. In part, this action reflected his desire to lay an organizational base for the changes of method and emphasis he had in mind. In part, it was dictated by the necessity of carrying out the provisions of the Daddario bill calling for inclusion in the hierarchy of NSF of a presidentially appointed Deputy Director and four presidentially appointed Assistant Directors.

Congress had specified the duties of the Deputy. He was to "exercise the powers . . . of the Director" in the event of the latter's absence or disability or when his office fell vacant. But Congress had not done this for the new high-level Assistants. It was left up to the Director to prescribe their duties and powers.

Making these decisions and finding people to take the posts required many months. Meanwhile McElroy wanted someone to help him frame his reorganization plans and see to it that they were put into effect. To this end he created a slot to be known as Assistant Director for Administration. Unlike the other new Assistant Directorships this was a career post as distinct from a statutory one. It was not subject to presidential action and could, therefore, be filled quickly.

The Director's choice for it was 43-year-old Bernard (Bud) Sisco, a veteran civil servant who had performed managerial chores for a variety of science-oriented Federal agencies, including the Naval Research Laboratory, the National Aeronautics and Space Administration, and the Office of Education at the Department of Health, Education, and Welfare where during a change of presidential administrations Sisco served as a Deputy Assistant Secretary. Obtaining his first rating at the age of 16, he had spent the remainder of his working life in civil service jobs except for 2½ years when, to use his own words, "I thought I wanted to be a cop and worked as a revenue agent."

He has said of McElroy's invitation to him to join NSF, "I was never offered a Presidential appointment and wouldn't have accepted one if I had been. I was a careerist with almost 30 years of service, and at that time I had every intention of staying around quite a bit longer. . . . I had seen the holy of holies and I knew it for what it was, a big empty room."

Whatever the nature of the room Sisco took over at NSF, he filled it adequately. Indeed when in 1972 he resigned to become McElroy's chief administrative officer at the University of California, San Diego, *Science* magazine quoted other officers of the Foundation as complaining that he had overfilled it. Noting that Sisco was on hand 6 months before the first presidentially appointed Assistants took office, the author of the *Science* piece—John Walsh—quoted some of them as saying that by the time they arrived at NSF Sisco had preempted so much authority that there was little left for them to assert.[18] When Sisco is asked today if he remembers John Walsh's critical words, he replies that they are "engraved in my forehead," tapping that impressively ample object as he speaks.

His comment on them: "I wish I had been that Machiavellian. I really do." He hastens to add that he considers Walsh's report a mixture of truth and fiction.

"I'll be very candid with you," he says. "I had a mandate from McElroy to institute the changes necessary to wake the Foundation up. . . .Now you don't sit down and design a Machiavellian plan that starts moving people around like chessmen out on a chess board. My job was to strengthen the organization, and to give the Director the ability to have an overview of everything that was going on, but also the ability to delegate to his line technical Assistant Directors [the Presidential appointees], and delegate not only responsibility, but authority, at the same time maintain enough control of the operation to be able to move back in when they got in trouble. I think it was this ability that I tried to provide for the Director (that he could move back in and participate in the direct management when there were problems) which a lot of people resented. I think a lot of people viewed the Director as the guy that was to sit up there and cut the grass and trim the trees and give 'em no lip. McElroy wasn't that kind of man . . . . He had made the decision that with one exception none of the then Associate Directors [the heads of the main NSF divisions prior to the McElroy shakeup] were to be presidential appointees, that he wanted a new team. That was not my decision. He told me that before he ever hired me. I think I was blamed to some extent for that decision, but it was not mine. . . ."

Asked what he found when he took over his duties at NSF in September 1969, Sisco replies, "a lot of nervous cats on a hot tin roof. I found people who were very uncomfortable with anything that didn't look like *laissez faire* and felt that the new broom was going to . . . sweep clean . . . everything they had worked so hard to build up, particularly . . . the role of NSF in supporting fundamental research in colleges and universities. . . .Believe me, that isn't what I wanted to do, and I don't think that's what Bill McElroy wanted to do. I think what he wanted . . . was to take that solid base and build on it—expand it, reexamine the priorities, find good and sufficient justification for increasing the fundamental research investment and at the same time move out into areas of more applied research . . . [and more] 'science in service to the Nation,' quoting Bill directly. . . ."

Having received his marching orders as it were, Sisco pushed them forward rapidly. He required the program directors to convene at intervals for the purpose of reviewing their plans for the year. It's his memory now that they fought this move "until they were blue in the face." He quotes them as saying, "What are you doing to us? What are you putting us through with all of this Mickey Mouse? This isn't NASA, and you're bringing in NASA techniques." But after the first program review, if we may trust Sisco's memory, they said "Gee, we learned a hell of a lot. If we weren't doing this for the Director, we should have done it for ourselves."

He studied the testimony offered by Haworth and his aides at NSF's first authorization hearings. He decided it was "pitiful . . . disjointed . . . it really never got the program across." With the assistance of another NSF official he saw to it that more work went into NSF's preparations for congressional hearings. The drafting of "detailed briefing books, background statements, . . . statements for the principal witnesses" became the order of the day. When these items were ready, their authors were required to practice them. Again it is Sisco's recollection that highly vocal resistance to this idea in the beginning was followed, after the first dry run, by appreciative acceptance of it.

One of Sisco's earliest discoveries about his boss was that McElroy wanted to be relieved of detail. Whereas during the Haworth regime members of the staff viewed the Director as doing too much himself, their assessment of his successor was that he pursued "a policy of total delegation." Sisco recalls McElroy saying to him, "For Christ's sake, they've got me signing every grant that goes out of this place! Get 'em off my desk. I only want to manage by exception." Sisco got them off. He assigned the job to his Grants and Contracts officer and supplied him with a signature machine. Simultaneously he devised a mechanism under which the Director, "anytime he wanted to," could find out what grants had been executed. "It doesn't make good sense," Sisco says, "for the Director to spend his time signing 12,000 grants a year."[19]

These and numerous other internal reforms were well underway before the long and trying process of selecting and clearing the new presidential appointees finally ended in 1970.

In April of that year the new Assistants took up their duties. They were physicist Edward C. Creutz, a vice president at Gulf General Atomic Company, as Assistant Director for Research; psychologist

Lloyd G. Humphreys of the University of Illinois as Assistant Director for Education; chemist Rear Admiral Thomas B. Owen, former Chief of Naval Research, as Assistant Director for International (later National and International) Programs; and biochemist Louis Levin of the NSF staff as Assistant Director for Institutional Programs. The organizational superstructure contemplated by the new charter reached completion in October when aeronautical engineer Raymond L. Bisplinghoff took his oath as Deputy Director. Then Dean of the College of Engineering at MIT, Professor Bisplinghoff for several years had occupied a series of top level administrative positions at NASA.

When McElroy took over the Directorship IRRPOS, the Foundation's first stab at implementing its new applied science powers, was not yet a fully going concern. On October 27, 1969, the Director gave it an organizational home by installing within Dr. Robertson's directorate an Office of Interdisciplinary Research. At Robertson's urging he put Joel Snow in charge of it—a logical choice in view of Snow's many contributions to the planning of the program.

Young Snow had come to this challenging and rather touchy job by what he himself describes as "unusual" pathways. During his high school days in St. Petersburg, Fla., he had defrayed his expenses by working for a local theatrical enterprise as a bit player, scenery mover, assistant stage manager, and chorus boy. He chose the University of North Carolina as his college largely because of its famous theater department, only to discover shortly that he had "soured" on the stage. At that he shifted to English literature, then to philosophy, and wound up his graduate work at Washington University in St. Louis with a doctorate in theoretical physics.

As the prime mover for IRRPOS he needed whatever he had learned from this variegated experience. For many months, save for one fulltime professional assistant, he did practically all of the work with the help of a temporary staff drawn from other elements of the agency. Theoretically IRRPOS was a pan-NSF endeavor. All divisions were supposed to supply ideas and assistance. In actuality they did so with what on the whole was a noticeable lack of enthusiasm. NSF's efforts to carry out its new powers had created within the agency a philosophical rift that is still visible there. To one side of it stood the many senior officials of the traditional scientific and educational units to whom the term "applied science" was a dirty word. On the other side were Snow and his small and shifting staff.

Not that IRRPOS lacked supporters or assets. It had plenty of both. As early as the fall of 1969 stirrings at the Bureau of the Budget (soon to be OMB) showed that that influential arm of the White House was interested in seeing more attention bestowed on socially relevant research by the Foundation. Equally helpful was McElroy's repeatedly expressed wish to enlarge that aspect of the agency's labors. Not the least of IRRPOS's assets was Snow himself. Bright, persuasive, eloquent, and by his own admission, brash, he lost no opportunity to preach the importance of "science in the service of the Nation." That the NSF traditionalists spoke of these performances as "snow jobs" can be taken as a sign that they found them difficult to ignore.

By the time IRRPOS got fully under way at the end of 1969, only 6 months remained in which to disperse the $6 million that Congress had grudgingly allotted for its first fiscal year. Robertson advocated holding the funding of grants to little more than a third of that, presumably fearful that hasty approval of proposals would put NSF in the position of supporting inferior work. From the Foundation's Advisory Committee on Engineering, however, came a fervent plea that all of the funds be committed by the end of the fiscal year (June 30, 1970) lest an already skeptical Congress lose faith in the program.[20] By the time this resolution appeared in March 1970, McElroy had long since reached the same conclusion. He ordered Snow and his aides to spend the full sum and they did. In a rush of work they considered some 200 proposals, accepted 21 of them, and by the close of the fiscal period had obligated $5.98 million.[21]

Meanwhile, during the preparation of NSF's requests for the next (FY 1971) budget, McElroy had asked $10 million for IRRPOS and OMB had lifted that figure to $13 million. This time it fell to Joel Snow to defend the new activity at the House authorization hearings. Chairman Daddario was still playing Doubting Thomas. He wanted to know why a program that was barely started needed such a large increase in funds. Snow's reply was that IRRPOS had evoked a "tremendous interest" on the part of the scientific community.

"Practically every major university has already been in to talk to us [about submitting proposals]," he said. "This does not mean just individual scientists, but . . . people from the top levels of administration all the way down through the faculty. Our problem is to keep people away from our doors long enough so we can get our day's work done."

The Chairman was unmoved. "Dr. Snow," he said. ". . . it will come as no surprise to you to know that there are still some cynics in the Congress." Daddario suggested that people were "pounding at your door" simply because they knew there was money there.[22]

No doubt the youthful IRRPOS leader breathed a sigh of relief when the $13 million request for FY 1971 survived the rest of the budgetary cycle.

When the time came to submit NSF's requests for FY 1972, McElroy simply repeated the 1971 request for $13 million. In view of his interest in IRRPOS, it may be asked why he suggested no increase. Perhaps his restraint was tactical in nature—an attempt to provoke the Office of Management and Budget into escalating the figure as it had done the year before.

As a matter of fact, OMB was about to go much farther in that direction than McElroy, when he submitted his requests to that body in October 1970,[23] had any reason to expect. Suddenly, dramatically, OMB presented the Foundation with a proposition that would have the effect of replacing the modest IRRPOS program with the far more comprehensive one now known as Research Applied to National Needs or RANN.

Chapter 13. New Faces and New Directions

[1] In Chron File: LJH, memo to NSB, September 4, 1968 (attaching draft of *Important Notice*, Subject: *Support of Applied Research*); LJH to Senator Warren G. Magnuson, October 11, 1968, and to Rep. Joe L. Evins, November 4, 1968.

[2] Daddario to LJH, December 6, 1968, Office of Deputy Director files.

[3] Letter, Randal M. Robertson to Mary Ellen Mogee, March 6, 1973 (cited hereafter as Robertson).

[4] Ibid.

[5] Advisory Committee for Engineering, Minutes, October 14-15, pp. 4-5.

[6] Robertson.

[7] Mary Ellen Mogee, "Public Policy and Organizational Change: The Creation of the RANN Program in the National Science Foundation," unpublished thesis, December 22, 1972, p. 42 (cited hereafter as Mogee).

[8] Robertson.

[9] Ibid.

[10] *1970 National Science Foundation Authorization*, Hearings before the [House] Subcommittee on Science, Research and Development . . ., 91st Cong., 1st Sess., Washington, 1969, Vol. I, pp. 229-240.

[11] *Science*, April 25, 1969; *New York Times*, April 17, 1969.

[12] *Science*, May 2, 1969, p. 532.

[13] Personal interview.

[14] *New York Times*, June 20, 1969.

[15] WDM to M. Brewster Smith, October 3, 1969, Chron File.

[16] Personal interview with Ryer.

[17] In Chron File: WDM to Handler, June 11, 1970; and memo to NSB, June 23, 1970.

[18] February 4, 1972, pp. 502-504.

[19] Personal interview.

[20] WDM to Winfield W. Tyler, April 22, 1970 (attaching memo from Division Director for Engineering to Director, Subject: Advisory Committee for Engineering Resolutions to the Foundation), Chron File.

[21] AR 1970, p. 121; *NSF—Grants and Awards, Fiscal Year 1970*, pp. 80-82, 260-70.

[22] *1971 National Science Foundation Authorization.* Hearings before the [House] Subcommittee on Science, Research and Development. . . . 91st Cong., 2d Sess., Washington, 1970, p. 172.

[23] WDM to George P. Shultz, October 6, 1970, Chron File.

Chapter 14

RANN

Of the several developments leading up to the creation of Research Applied to National Needs (RANN), the most significant took place at the White House in 1970. Heretofore the Nixon Administration had stressed austerity in governmental spending. The late fall of that year, however, witnessed a shift of policy with the President requesting that, in the interest of stimulating the economy, ways be found of letting the Federal agencies spend more money during the next (FY 1972) budgetary year.

At the Office of Management and Budget officials concerned with NSF saw in the President's order an opportunity to effect what they viewed as badly needed changes in the Foundation's funding practices. According to one such official, Carlyle E. Hystad, OMB believed that NSF was devoting too large a proportion of its funds to educational and institutional endeavors. At that time the Foundation's educational programs still ranged from extensive assistance for graduate students to the promotion of projects aimed at modernizing science curricula or at upgrading the skills of science teachers. Its institutional activities still provided block grants that colleges and universities, or departments of them, could use for improving their science-training programs and facilities.

"Our feeling," Hystad has said, "was that maybe those emphases were proper in the late 1950's and early 1960's, but by 1970 the situation over the country dictated a new spending pattern. The buying power of the research dollar was falling. We had a glut of Ph.D.'s on the market as a result of a downturn in hiring by the space program and the Department of Defense along with a leveling off of activity on the part of other users of scientific and engineering manpower." OMB's position, in short, was that the time had come "for the Government to spend less on the production of new scientists, more on helping those who already had their training and were out there waiting for jobs." Asked if the annoyance of the Nixon Administration at the campus disruptions of the 1960's was a factor in its wish to reduce the Foundation's expenditures for fellowships and traineeships, Hystad says "I doubt it." He points out that "generally speaking" the recipients of NSF awards, "most of them students of science on the graduate level, were not the campus activists."

A corollary of OMB's pragmatic reasoning was its desire that the Foundation expand its support of applied research. Hystad says that he and his colleagues found it difficult to sympathize with fears that this "shift of focus" would destroy NSF's status as a "pure research agency." By 1970, he says, the "purity" of the agency was already diluted. Early in its career, he notes, the Foundation had been given the opportunity of choosing between scientific purity, on the one hand, and budgetary growth, on the other.

"Long before the Daddario bill," he argues, "even before Haworth took over, the Foundation opted for growth. It did so by bowing to demands (from both Congress and the President) that it take on a substantial amount of practical work. I cite you, for example, the agency's support of applied research in connection with such activities as its atmospheric and marine programs."

He concedes "that there's a real question of whether NSF should be in the applied research area. You can debate that," he observes, "You can make some strong arguments for both sides. But I don't think the issue was still debatable in 1970. By then the Foundation, of its own volition, had become a diversified agency. Were its leaders sincere about touching nothing but pure research, they should long since have settled for a budget in the neighborhood of $200 million a year."

It will be recalled that for fiscal year 1971 McElroy had requested $10 million for IRRPOS and that the officials at OMB had lifted that figure to $13 million. This action did not imply any great affection on their part for the new NSF program. Far from it. They were convinced that the Foundation had not devised a mechanism capable of pinpointing the national difficulties to which the agency should address itself. Hystad's recollection is that where the new program was concerned, "we had the impression that all that was happening at the Foundation was this: Whenever the program directors received a proposal that cut across disciplinary lines, they shunted it to IRRPOS for action." That was not OMB's idea of a proper procedure. Its leaders wanted less emphasis at NSF on breaking down disciplinary barriers, more emphasis on problem solving.[1]

It is imperative at this point to interject a brief comment on these attitudes. An examination of the manner in which IRRPOS was shaping up toward the end of 1970 strongly suggests that the OMB people, in their appraisal of the program, tended to exaggerate its weaknesses. Where both procedures and the selection of projects to support were concerned, IRRPOS was already moving in the direction that officials at the Office of Management and Budget thought the Foundation's applied-research effort should go. Obviously, however, it was not moving fast enough to suit them. Hence the vigor with which they set out to initiate some drastic changes at NSF when in the late fall of 1970 the White House called for an increase in governmental spending.

By this time the FY 1972 budget cycle was well advanced. Indications were that the Foundation would be allowed to ask for $530 million.[2] True, McElroy had proposed a much larger figure, but he had resigned himself to that one, realizing, as he wrote later, that "Man proposes, OMB disposes."[3] It can be assumed that he was not displeased when in early December the Office of Management and Budget let it be known that NSF would be permitted to increase its appropriations request by $100 million.

* * * * *

As has been indicated, some terms were attached to this rather startling offer.

One of OMB's conditions, subject as all of them were to Congressional approval, was that the Foundation terminate its institutional activities. Existing agreements, calling for installment payments in the future, were to be honored; but no further grants under the institutional directorate as such were to be made. Indeed the directorate itself was to be abolished.

Other conditions contemplated some changes of thrust and tone in the Foundation's support of education. All arrangements for the distribution of fellowships and similar awards were to be reduced or discontinued. Covered by this proviso was a traineeship program for first-year graduate students that was already moribund as the result of presidential impoundment of the $9.5 million allotted to it under the current budget.[4] All activities aimed at sharpening the skills of science teachers, including the summer institutes, were to be reduced or phased out. Textbooks and other training material generated with NSF funds were to wear a new look. Past policy had been to direct most of such items to "the best and the brightest" among the science students in the country's high schools and colleges. Henceforth, a greater effort would be made to produce material suitable for students in the lower ability ranges or for those, whatever their ability, who were interested in learning something about science but not in pursuing it as a career.[5] Future activities would also include programs specifically designed to upgrade the science components of small colleges and of those attended chiefly by black students.

Still another condition was that NSF solicit congressional sanction for a substantial increase in its funding of projects in the basic sciences.[6]

As for the additional $100 million, the Foundation was to bestow about half of that on the enlargement of its applied-research program.

It was to devote the other half to its attempts to cope with what NSF officials were then speaking of, rather dolefully, as the "Mansfield effect."

The Mansfield effect took its name from an amendment to the 1970 military procurement law which, although introduced by Senator J. William Fulbright, has always carried the name of its staunch proponent and framer of an identical provision, Senator Mike Mansfield of Montana.[7] The amendment made it unlawful for the Department of Defense to finance any projects or studies in basic science unless, in the opinion of the Defense Secretary, they were clearly related "to a military function or operation." Stressing the need for more Federal support of such research, a related clause of the act ordered NSF to provide a "larger share" of it.[8]

Even before the Mansfield amendment became law on October 7, its provisions had created a problem for the Foundation. Although the amendment mentioned only the Department of Defense, other governmental funders of basic research—notably NASA, the Atomic Energy Commission, and the Department of Health, Education, and Welfare—had got the message. By the close of 1970 NSF was finding itself hard put to dig up approximately $40 million for the support of scientifically valuable programs that were in the process of being dropped by the mission agencies.[9] Even more difficulties of this sort lay ahead with McElroy predicting that about $74 million of the Foundation's upcoming budget would be needed to take care of research projects scheduled for elimination by other agencies.[10]

An indication of the importance that the leaders of OMB attached to their $100 million offer in December 1970 can be seen in their decision to work out the details in a face-to-face conference between one of their senior officials and the Foundation leadership. Invitations from the Office of Management and Budget to such high-level meetings have not been a commonplace of NSF history. Prior to 1970 only one took place. That, it will be remembered, was in 1954 when the Director of what was then the Bureau of the Budget and two of his aides called at Foundation headquarters in a futile effort to persuade Alan Waterman to take more seriously the coordination chores prescribed by the original NSF charter. A certain irony clings to the failure of the budget bureau at that time to achieve its ends. So far, it would seem, soft-spoken Alan Waterman has been the only NSF Director to tell the most powerful agency in the Executive Office to go jump in the lake—and get away with it.

Hystad of OMB underscores the significance of the 1970 OMB-NSF confrontation. "Each year," he explains, "each agency submits its budget requirements for review here. We make recommendations which then go to the President who approves or changes them. That done, the appropriate OMB official calls up the agency head and says something like, 'Come on now, let's have a few chats about what the President did to you.' Sometimes the chats involve only phone calls and correspondence, but if the agency is a major one they usually include a personal conference as well." According to Hystad, the OMB-NSF conference on December 13, 1970 was unusual in that OMB regards the Foundation as "a little more than a minor but not quite a major agency."

It is his memory that the session took place in Room 248, the OMB Director's conference room in the (Old) Executive Office Building next door to the White House. Starting time: 5:30 p.m. Duration: 45 minutes. On hand for OMB, its Assistant Director, Donald B. Rice; for the Foundation, McElroy and Bud Sisco. Also present, Dr. Edward E. David, Jr., specialist in communications technology and computers, who had only recently succeeded DuBridge as science advisor to the President and head of the Office of Science and Technology. Purpose of the conference: To make sure that the Foundation understood and accepted the conditions under which OMB stood ready to permit the agency to add $100 million to its current budgetary requests.

It would appear from McElroy's correspondence file that he was not happy at the prospect of closing out the Foundation's institutional efforts. Hystad's memory, however, is that he voiced no objections to doing so at the December 13th conference—a recollection that seems to bear out Dave Ryer's statement that one of McElroy's attributes as an administrator was that "he knew when to fight—and when not to fight." By the end of the session, the Foundation had accepted all of OMB's conditions and, according to Hystad, the NSF Director "was smiling when he left the room."[11]

One of the agreements crystallized that afternoon was that the Foundation would not content itself with simply reforming the IRRPOS program. That entity was to go and a different one be put in its place. Another understanding was that the successor to IRRPOS would not be a part of the Research Directorate. It would function under separate management in deference to the conviction at OMB that the passive operational methods of the Research Directorate were inappropriate for any program set up to handle socially relevant research. The new program was to be managed in an aggressive manner. Those in charge would try to identify specific societal problems, locate the requisite experts and make certain that they hewed to agreed-upon objectives.[12] A further understanding was that work on the new program begin at once so that the plans for it could be shown to Congress during the spring authorization hearings.

Whether McElroy can be said to have bypassed the National Science Board on this one is a moot question. With the next meeting of that body scheduled for December 17, he had to work fast. In a memo for the Board, dated December 15, he outlined his plans. His major proposal was that most of NSF's existing problem-oriented activities, including IRRPOS, be brought together under a newly created and newly named directorate of the Foundation to be headed by an Assistant Director equal in rank to the other four Assistants then in office.

Aside from this concrete suggestion, the memo dealt for the most part in generalities. Much of it consisted of arguments calculated to quiet the fears of many Board members that an enlargement of NSF's role in applied research would detract from its position as the country's preeminent guardian of basic research.

One of McElroy's points was that most of the problem-oriented work supported by the Foundation would involve a large amount of basic science. His reasoning was that more often than not those responsible for tackling a social problem would find that some of the fundamental knowledge required for its solution was not yet on hand. It followed, he reasoned, that "a determined assault on an important area of applied research could form an umbrella under which increased support of relevant basic research would take place."[*]

The Director took pains to point out that NSF had no intention of taking on projects that the Federal mission agencies were in a position to pursue. He described the proposed new program as a "pilot plant." As such it would not only break "new ground in the application of science to [the] solution of society's pressing problems," but also develop "the kinds of institutions and educational processes most effective in achieving this end. The Foundation," he added, "can be expected to play this role in areas not directly within the charter of an individual mission agency . . . and also in areas where [such] an agency is ill equipped to handle the science and technology aspects. . . ." As each endeavor of this kind neared success, it would be handed over to the appropriate mission agency for further research and possible development.

In his memo to the Board McElroy did not specify precisely how much money he hoped to spend on the new program. Later, after the December 17th Board meeting, he would convey this figure—$81 million—to the NSB chairman by phone. He did note in his memo that he thought it "not unreasonable to expect that up to 25 percent of an expanded NSF budget could be devoted to the new program." In a burst of McElroyan optimism, he wrote that:

---

[*] From Bud Sisco comes an interesting comment on this suggestion by McElroy that by taking on more applied research the Foundation would be able to increase its support of basic research. "I came to NSF out of NASA," Sisco recalls today, "where we did one hell of a lot of fundamental research under the cloak of applied science and applied engineering. We supported the whole Goddard Space Flight Center [home of a variety of basic-science activities, ranging from solar physics to intensive study of fields and particles] . . . under the mantle of the applications program. We didn't do the applications program because we necessarily loved it; we did it because that way we got money that enabled us to support fundamental research. I think . . . that was the situation at NSF as Bill McElroy saw it . . . ."

"Beyond the solution of pressing national problems, there is an important continuing role for the National Science Foundation in contributing to the growth of the Nation's productivity. Science and technology, more than ever before, will be called on to keep productivity in the service economy, as well as the production of tangible goods, rising fast enough to overcome the unforeseen diseconomies in our Nation's institutions and to satisfy the needs for higher personal income and reasonable profits without steady inflation of prices."[13]

If any Board members voiced objections to McElroy's ideas during their December meeting, the record fails to show them. All the minutes reveal is that the "Board approved the Director's general organizational and program plans for expanding Foundation support in applied areas, as authorized by the 1968 amendments to the NSF Act."[14]

* * * * *

Once McElroy had the approval of the Board, he could move ahead. His first step was to place the responsibility for developing the new program in the hands of Ray Bisplinghoff, his Deputy Director. Bisplinghoff in turn instructed Joel Snow to assemble an ad hoc committee, a task force, and draft the plan.

Snow today has vivid memories of the circumstances under which he received his orders in the Director's office. Bisplinghoff was standing by as McElroy said to Snow, "I've got a job I'd like to have you take on. Here it is," handing him a piece of paper, probably the memo that had been prepared for the National Science Board. "Oh, by the way," McElroy added, "I'm going on vacation for 2 weeks and you'll be working with Ray." The implication was that the Director expected to find a draft of the plan on his desk when he returned. Bisplinghoff took no chances. His orders to Snow were to have the plan ready 1 week hence.

This conversation occurred on December 21. On the 23d Snow and his task force—seven other staff members—sat down at a conference table. Snow's first job was not a pleasant one for him. He was proud of IRRPOS. It was largely his creation, and he thought it had done a pretty good job of getting "an interdisciplinary problem-solving capacity established in the universities." He would like to have said, "Boys, we're going to touch up IRRPOS a bit." Instead—after all, his instructions on this score were explicit—he had to tell them that their task would be to destroy IRRPOS and replace it with a more elaborate arrangement.

What should the new program be called? During the next few days Bisplinghoff and Snow and his task force brainstormed that question repeatedly. One of Snow's earlier suggestions was "Applied Research and Problem Assessment," but the acronym yielded by those words, ARPA, not only sounded like a dog's bark but already had been used elsewhere in the Federal bureaucracy.* From the Office of Science and Technology came another suggestion, "Research Applied to National Development." That one was ruled out because its acronym, RAND, was the name of the well-known think tank, the RAND Corporation.

At some point someone suggested "Research Applied to National Needs," a name that McElroy regarded as a semantic triumph in that the phrase "research applied," as distinct from "applied research," clearly indicated that RANN would support whatever kind of science—basic or applied—the problems with which it dealt appeared to require. RANN officials now estimate that about 40 percent of the funds allotted to the program are spent on the search for new scientific knowledge.

One of the developments connected with the origins of RANN can be cited as a measure of the Foundation's reluctance to abandon its institutional program. There was a strong feeling at NSF that the block grants that this program made available to academic departments of science had produced some extraordinarily valuable results. Consequently Joel Snow was not surprised when Bisplinghoff asked him and his task force to analyze some of the requests for such grants then under consideration, on the possibility that they could be legitimately funded under RANN.

Talking now of Bisplinghoff's suggestion, Snow says that the successful effort to salvage portions of the institutional program in this manner is "one of the things that has been overlooked in most of the writing about both RANN and its predecessor, IRRPOS." He points out that when in early 1970 IRRPOS "showed signs of generating a lot of political support," the people in charge of the institutional program began making some changes in their work. What they did was to set up within the institutional directorate a special unit known as the Division of Institutional Development. Ably headed by William V. Consolazio, a many-talented veteran of the senior staff, the new unit carried on negotiations aimed at so revamping a number of requests for institutional grants that they could be accepted and funded as problem-oriented projects. By the time the RANN planners began their work in late December, Consolazio's efforts were well advanced; and, in the near future, seven undertakings, originally proposed as institutional projects, would be underwritten by RANN funds.[15] Most of them were 3-year projects and Snow says today that as they terminate, "we're going to take a close look at their accomplishments. I think that several of them have made better use of their money than some of the regular [RANN] research projects have."

---

* By the Advanced Research Projects Agency, a short-lived unit of the Department of Defense.

The task of drafting the original plans for RANN took place during the 1970 Yuletide. Both Snow and one of the members of his task force, Dave Ryer, now recall spending Christmas Day scribbling and typing on long pieces of yellow foolscap. Time was of the essence. The planners would have found their deadline (December 28) even harder to meet had it not been for the assistance provided by several outside organizations and especially by the National Academy of Engineering.

In the spring of 1969 the Foundation had asked the Academy's Committee on Public Engineering Policy to suggest areas of applied research that NSF "might begin to support." Polled by COPEP the members of the Academy had submitted 700 suggestions. These the committee had collected under seven major headings, ranging from projects aimed at alleviating air and water pollution to the finding of ways to apply computer techniques to medical diagnoses and systems of ambulatory care.

Draft copies of the two of what would eventually be three COPEP studies were made available to the RANN planners. They were heavily used. "We'd put down everything we could think of," Snow recalls. "Then we'd check our ideas against the COPEP lists." COPEP also offered suggestions for creating a managerial structure suitable to the objectives of RANN. These, too, were frequently consulted.

Snow says *mea culpa* when his attention is called to the statement in an unpublished study of RANN that now and then he and his fellow planners resorted to a little "creative writing."[16] According to Snow they had to. McElroy's orders were that they were not to discuss what they were doing with other elements of the Foundation. As a theoretical physicist, Snow found this constraint particularly hampering during his efforts to suggest means for using the social sciences in the treatment of national problems. He would like to have sought the help of NSF's social science experts but his instructions precluded that action.

Why this cloak-and-dagger reticence? A document subsequently drafted in connection with preparations for presenting RANN to

Congress shows that the NSF Director had his reasons. He was afraid that if the planners' activities became common knowledge, there would be squawks from the anti-applied research segments of the scientific community and possibly from some of the other Federal agencies as well. Such protests, he feared, would put obstacles in the path of the planning process. Recalling the constraint on the RANN task force and his dislike of it, Snow says today that he assumed that it was invoked at the request of the Office of Management and Budget. Hystad says no. His recollection is that, on the contrary, "we were pushing them [NSF] . . . to get everybody they could within the Foundation to come up with ideas . . . ."

On "D-day," December 28, Snow delivered the plan for the new program to Bisplinghoff. McElroy, on returning to his desk in early January 1971, reviewed it, "signed off on it," and in a warm note to "Dear Joel" thanked the ad hoc committee for "a first rate job."[17]

From this point on the movement of RANN from plan to reality was swift. On February 1, a staff memo proclaimed its existence and announced that a new major division of the Foundation, to be known as the Research Applications Directorate, was being organized to manage the program. Pending presidential appointment and Senate confirmation of a fulltime head for the directorate, Bisplinghoff would remain in charge.

On February 23, at Bisplinghoff's request, arrangements were made for the Director to release $11,448,000 of his $12 million special reserve fund to initiate the first seven projects under the new program. RANN, in short, was well on its way to operational status when 2 days later the Director and the Chairman of the National Science Board went up the Hill to testify at the opening session of the NSF authorization hearings in the House of Representatives.

Among the grants financed out of the Director's reserve fund was one for $650,000 to the State University of New York at Albany for a study in criminal justice dealing with the role of deterrence in criminality and related matters. Also included was an award of $3.1 million to the University of Michigan for a study of the quality of American life for the purpose of developing a "system of social indicators."

Some of these initial RANN grants had begun life as requests for institutional funds, Consolazio and his colleagues, in consultation with the requesting universities, having transformed them into problem-oriented proposals. Such was the background for a $1.2 million grant to the California Institute of Technology for a 3-year "attack on critical environmental problems affecting modern society." Some of the findings of this project would prompt the California legislature to change the State's laws in regard to air pollution requirements in the Los Angeles basin. Others would stimulate the development along the West Coast of geothermal energy resources.[18]

On March 2, a press release announced the appointment of Dr. Alfred J. Eggers, Jr., to become NSF's first Assistant Director for Research Applications. Coming to NSF a few weeks later, after many years in high-level posts at NASA, Eggers would be assisted by Snow, as Deputy Assistant Director for Science and Technology, and Leon M. Schwartz, as Deputy Assistant Director for Program Management.

On March 5 a staff memo revealed the branches through which the new Directorate was to perform its functions. An Office of Intergovernmental Science Programs would assist "State and local levels of government to develop organizational concepts and procedures" for enhancing "the application of science and technology to governmental problems." An Office of Exploratory Research and Problem Assessment would unearth "national problems . . . amenable to solutions through the aggressive application of science and engineering capabilities." The RANN program proper would begin life with three working units: Social Systems and Human Resources, Advanced Technology Applications, and Environmental Systems and Resources.

On March 11, during a conference with the staff of the Committee on Science and Astronautics (the House authorization unit), RANN officials dwelt at some length on the rationale underlying their recently created program. Their presentation was in the nature of a defense, for by the date of this meeting the House authorization hearings had proceeded far enough to indicate that RANN was in trouble.

Beginning on February 25, 1971 the House authorization hearings continued intermittently through March and into April. In 1970 the Subcommittee on Science, Research and Development had conducted these proceedings. This year the full committee was on hand with its chairman, Representative George P. Miller of California, presiding.

From the first session on it was clear that the attitude of most of the members toward the new priorities set forth in the budget requests under consideration was one of skepticism heavily tinged with dismay. One of the earlier expressions of this mood came from Republican Congressman Charles A. Mosher of Ohio, a knowledgeable and longtime supporter of the objectives of the National Science Foundation. Mosher directed his remarks at the top NSF leaders, McElroy and Dr. Herbert E. Carter, a chemistry professor at the University of Illinois,* who in 1970 had succeeded Phil Handler as Chairman of the National Science Board.

"Gentlemen," Mosher said, ". . . your budget proposals indicate . . . almost revolutionary new directions for the work of the National Science Foundation, increased emphasis on research in the social sciences and very heavy emphasis on applied research. It would appear that you are almost moving into a mission-type agency in some respects, and obviously . . . we are going to be . . . exploring in depth these philosophies and the necessity for these changes. . . . I share some of the concern that has already been expressed here concerning [the] reductions proposed in institutional support for science and in the science education support."[19]

Daddario was no longer present, but quite clearly many of the members of the authorization committee agreed with his often expressed feeling that the fate of the Foundation was too important to be left to the care of the executive branch. When on March 27 NSF Assistant Director Humphreys spelled out the extent of the proposed cuts in the support of education, Congressman Earle Cabell was quick to take note of what seemed to him to be an incredibly swift reversal of position on the part of the Foundation. The Texas Democrat pointed out that only a year earlier NSF had been singing the praises of the very programs it now proposed to cut or discard. Should this change of tune be taken to mean that the Foundation "had no faith" in its own programs? Or should it be regarded as a case of NSF submitting to "a mandate from the Office of Management and Budget?"[20]

At the last session of the hearings, on April 7, the doubting committee members found that they had an ally in Phil Handler. Speaking not as an NSB member but as President of the National Academy of Sciences, Handler described himself as pleased to see that the Foundation was asking for an increase in the funds for the support of basic research projects.

"On the other hand," he added, "I cannot support the manner in which the funding is being generated." He reminded the lawmakers that the National Science Board, in its first annual report to the president in 1969, had stressed the importance of fellowship and traineeship programs and of block-grant support for universities, many of which, according to NSB, were "in such fiscal difficulty" that they could not survive without such support. "In summary, then," Handler said, "I consider that taking money [from NSF's educational and institutional programs] . . . is a trend in the wrong direction." He foresaw the agency's support of graduate education as "on the road to extinction if the . . . recommendations in this year's budget are indeed implemented. I hope that will not be the case."

---

* Now Coordinator of Interdisciplinary Programs at the University of Arizona in Tucson.

Handler's words impelled Congressman Cabell to repeat a question. "I have tried now for a number of occasions," he said, "to find out whether the reversal of policy came from the National Science Foundation or was a mandate from [OMB]. . . . Would you care to respond to that?" Handler said he "would not care to respond" and Cabell said resignedly, "I thought so."[21]

On May 30 the authorization committee issued its recommendations. Never mind what OMB wanted. The committee wanted the educational and institutional funds to be restored. On the other hand it did not want RANN to have all of the $81 million requested for that operation. The committee's reasoning was that NSF was moving too fast in the direction of applied research.

"In spite of the very large sums being expanded on applied research by the mission agencies," it reported to the House, "and in spite of the foregoing legislative history, applied research in the NSF budget has advanced from 0 percent* to more than 11 percent of the total NSF budget within 2 years. . . . Moreover, the Foundation's Director has testified that such research may eventually run as high as 20 percent or 25 percent of the NSF research budget."[22]

As the Committeee on Science and Astronautics bent so the other congressional bodies concerned with the funding of NSF inclined. The Federal budget, when it finally materialized during the summer, restored the Foundation's educational and institutional funds—an academic provision, to be sure, since FY 1972 would be marked by presidential impoundment of the bulk of these restored sums. The total for the Foundation—$622 million—was precisely the amount that the President had requested and its largest budget to date. As for RANN, its share came to $54.1 million, $30 million less than had been asked for.

The cut compelled the RANN officials to do some swift revising of their plans. Even so the new program was solidly entrenched at NSF when on July 19 McElroy announced his impending resignation to become Chancellor of the University of California at San Diego.[23]

Once again the National Science Board turned to the President of Carnegie-Mellon University. Two and a half years before H. Guyford Stever had refused to be considered for the NSF Directorship, pleading unfinished business at his Pittsburgh campus. This time he pronounced the business there well in hand and expressed an interest in a change of job. On February 1, 1972, having been named by the President on November 15 and subsequently confirmed by the Senate, he embarked on his duties as the fourth Director of the National Science Foundation.

---

* The committee's "0 percent" overlooked some funding of applied research under a variety of long-standing NSF programs.

Chapter 14. RANN

[1] Personal interview with Hystad.

[2] WDM to Shultz, October 6, 1970, Chron File.

[3] WDM to Handler, December 2, 1971, Chron File.

[4] WDM to Senator Harrison A. Williams, Jr., February 3, 1971, Chron File.

[5] In Chron File: Memo from Division Director, Pre-College Education in Science, March 2, 1970; letter, WDM to George B. Brain, March 16, 1970.

[6] Mogee, p. 64.

[7] Rodney Nichols, "Mission-Oriented R&D," *Science*, April 2, 1971, p. 29.

[8] Public Law 91-441, 91st Cong., H.R. 17123, October 7, 1970.

[9] *1972 National Science Foundation Authorization*, Hearings before the [House] Committee on Science and Astronautics and the Subcommittee on Science, Research and Development . . . 92d Cong., 1st Sess., Washington, 1971, pp. 32, 384.

[10] WDM to Handler, May 3, 1971, Chron File.

[11] Interview.

[12] Mogee, p. 66; interview with Hystad and Joel A Snow.

[13] NSB-70-336.

[14] NSB Min. 134:2.

[15] Consolazio, memo to Snow, December 8 1971, Office of Deputy Director files.

[16] Mogee, p. 71.

[17] January 5, 1971, RANN files.

[18] Memo dated February 22, 1971, RANN files.

[19] *1972 National Science Foundation Authorization*, Hearings. . . ., p. 45.

[20] Ibid., p. 434.

[21] Ibid., pp. 699-701.

[22] *House Report on H.R. 4743*, p. 71.

[23] WDM, memo to all employees, July 19, 1971, Chron File.

Chapter 15

The Fourth Director

The appointment of H. Guyford Stever brought to the helm of the Foundation a forceful and likeable executive with a penchant for apt witticisms and what appears to be a high degree of unflappability—an attribute that Stever has no doubt found useful in coping with the problems, some of them stemming from Congress and others from the White House, which have beset the Foundation since his assumption of the directorship in February 1972.

A native of Corning, N. Y., where he was born on October 24, 1916, Stever grew up for the most part in the home of a relative, owing to the loss of both parents soon after the first World War. By the time he finished high school the depression had set in. Money was close to nonexistent, but he was able to enroll at nearby Colgate University, thanks to the procurement of a scholarship for him by the Superintendent of Schools in Corning. No money was forthcoming under the scholarship pending the completion of his first semester at Colgate. Fortunately, young Stever had saved $175 from his paper route. That covered tuition. Jobs provided by the school took care of room and board, and in the summers he went home to work in the packing and shipping department of the Corning Glass Works.[1] In later years he would recall being at the works during the great flood of 1935 when "we all worked overtime" to protect a 200-inch mirror then being readied for use in the optical shop at California Institute of Technology.[2]

Science had intrigued him since his early teens when some telescopes donated to his Boy Scout troop excited in him a curiosity about the wonders of nature.[3] At Colgate he majored in physics with minors in mathematics and chemistry. He did well enough to be named valedictorian of his class in 1938 and to elicit acceptances from five first-rank graduate schools. His decision to follow the 200-inch mirror to Caltech was prompted by what he now speaks of as the "personal touch" in a letter from the then Chairman of the Executive Council there, Nobel laureate Robert A. Millikan.

For his doctoral thesis he measured the lifetime of a meson, a subatomic particle whose discovery in the 1930's has helped investigators in their efforts to comprehend the behavior and properties of the cosmic rays that stream into the terrestrial atmosphere from outer space. One of his fonder memories of Caltech is of a visit to his research room one day by Robert Oppenheimer. The famed atomic scientist, Stever recalls, "lectured to me all afternoon. I took notes at a high pace, and it took me a semester to understand what he had explained to me."[4]

The receipt of his Ph. D. in 1941 put him to the task of choosing between an offer to teach physics at Stanford and one to do war research at Massachusetts Institute of Technology. He chose the latter, spent a year at the Radiation Laboratory at MIT, and 3 more years (1942-45) as science liaison in London for the Office of Scientific Research and Development.

The next 20 years found him on the faculty at MIT where he combined a diversified teaching program with numerous jobs in Washington for the Federal Government. From 1955 to 1956 he was Chief Scientist of the United States Air Force. In 1959 he became a member of the Management Advisory Panel of the Committee on Science and Astronautics in the House of Representatives. He was head of MIT's departments of mechanical engineering, marine engineering, and naval architecture when in 1965 the call came to the presidency of the Carnegie Institute of Technology, from which position Stever (in 1967) oversaw the merger of that 62-year-old school with the younger Mellon Institute to form what is now Carnegie-Mellon University.

Obviously Stever's career has been characterized by considerable variety as distinct from the tendency of the average scientist, especially in his younger years, to concentrate on some aspect of his field. If it can be taken as a sign of the times that he is the first NSF Director to have a primarily engineering background, it can also be set forth as significant that he appears to be more of a generalist than any of his forerunners. The broad-gage approach to the promotion of research and education in the sciences is congenial to him. It is therefore hardly surprising to find him in accord, generally speaking, with the changes of emphasis effected at the Foundation under Bill McElroy.

Like his immediate predecessor Stever sees in these new directions no threat to the effectiveness of the Foundation as a champion of pure science. He has no patience with what he speaks of as the "either/or" approach to the basic-science versus applied-science argument.

"There are some people," he has said, "who . . . feel . . . that basic research is worthless—literally. But, fortunately, they're in a minority, and when they raise their voices they don't have great power. And I'm quite sure there are some people who feel that basic research should never be tainted at all with application. But they don't carry the day either.

"There . . . are people in science who get their motivation out of the pursuit of knowledge. They work best when left on their own, allowed to come up with their own ideas. Many of those ideas will push their science ahead. By pushing science ahead we will get a practical payoff eventually. Lots of very practical people in government believe in that process of science.

"There are other scientists who get their motivation when they have a specific goal. I think our job is to tap both kinds."[5]

In a May 1973 letter to an alarmed colleague, Stever wrote that concerning "the potentiality of applied research driving out or minimizing the national effort in basic research, I appreciate your apprehension even though I don't believe there are grounds for it. As you undoubtedly realize, science has got to come to grips with the current problems of the Nation and society, and that, in a small way, is what the Foundation is attempting to do. But we are not pursuing the applications route without adequately supporting the basic sciences. The Foundation's primary mission is to strengthen the basic sciences, and that we are continuing to do. It is true that the rate of growth of the basic sciences has slowed, but it is equally true that the exponential rate of growth in the 1960's could not continue for long."[6]

Especially revealing of his administrative philosophy is another 1973 communication in which Stever expressed regret "that long-range support of research is being sacrificed to short funding," but contended that in view of current financial constraints on Federal spending, "the alternative is even less acceptable—longevity for the few and nothing for the others." He added, however, that he was "fully convinced . . . that basic research will not suffer. There will be some growth but the rate will be different, and not all areas will grow at the same rate. I am also convinced that if there is any great expansion in basic research, it will occur in those areas . . . that underpin the 'national needs' issues. It is up to those of us who help establish national policy and priorities to see to it that the mix between science-generated and problem-oriented basic science is just right; that is the principal question I see facing the scientific community, the Foundation, its Board and Director for the next decade."[7]

Discussing these matters in the quiet of his office, Stever twirls the propellor of a model airplane around a sturdy index finger, sending a stutter of static into the tape recorder, as he surveys a recent stretch of history in support of his points. He notes that during the decade and a half beginning about 1950, Government funding of science grew by leaps and bounds. During that same period the country's stockpile of fundamental knowledge enlarged enormously. He cannot bring himself to argue with what he perceives to be a wish on the part of the American people at this point to see a greater effort put forth to apply some of these accumulated data to the solution of national problems.

Under Stever the Foundation's applications programs have prospered. RANN's share of the agency's $645.7 million appropriation for FY 1973 was $70 million, an increase of $16 million over its initial allotment the year before. The program's share of the FY 1974 appropriation, $704 million in all, was $75 million. The figures for FY 1975 were $768.8 million for the Foundation and $143.4 for RANN, but during the year $51.7 million of RANN's funds were transferred to the newly activated Energy Research and Development Administration. For FY 1976 the total for the Foundation was $714 million and for RANN $60 million.

Notable among the changes that Stever himself has initiated are a number of shifts of focus aimed at healing what he describes as a long-standing rift between the country's institutions of higher education, on the one hand, and such important elements of society as industry and local governments, on the other. Stever recalls having had his eye on this situation even before his arrival at the Foundation, and of having set out at once to do something about it on taking over the Directorship.

He dates the beginnings of the rift to World War II days. He attributes it to the constant flowing into the colleges and universities, for many years thereafter, of what he calls "idea money from the Federal Government." As a result of this "infusion" of Washington dollars, the campuses tended to move away from a concern with the interests of industry and local government in the direction of an almost unbroken preoccupation with matters of a national or global nature.

"It soon became so easy at the campus level to get support for any idea that fitted . . . the national mode," Stever says, "that no one even tried any other modes. As a consequence a real disaffection arouse between academia and industry."

On coming to the Foundation he was gratified to see that the RANN program was already sponsoring a number of interdisciplinary projects involving cooperation between a university and one or more commercial firms or local governments. The second NSF charter permits the agency to support problem-oriented applied research in industry when directed to do so by the White House; and in the spring of 1972, shortly after Stever took office, the President delegated this authority to the NSF Director.[8] At RANN many such arrangements take the form of projects managed by an educational institution with industry furnishing some of the facilities and expertise. In a fair number of instances the project is handled jointly by a university, an industry, and a local government.

A good deal of NSF's attempt to lower the barriers between academia and industry has taken place in its educational directorate. Here the alteration in the support pattern has been away from training programs designed to produce research-based Ph. D.'s and in the direction of programs designed to prepare and motivate science students in the graduate schools for work in industry or in other nonacademic environs.

Stever views this change as a common sense response by the Foundation to the changes that have overtaken American institutions of higher education in recent years. He recalls that during the plush decade and a half that began in the 1950's university enrollments increased phenomenally. During this period the universities themselves became the chief consumers of their own Ph. D.'s. More often than not a science student, after getting his degree, simply went on doing basic research at any one of the several universities willing and able to hire him. Relatively few headed for industry because "the motivation was all in the academic direction."

Now, he points out, the situation in the universities has changed. Although the student population, after standing more or less constant for a few years, has begun to rise, financial difficulties make it impossible for the universities even to begin to absorb all of their good science Ph. D.'s. The burden of providing employment for them has shifted to industry and other nonacademic entities. "And the Foundation," says Stever, "is trying to reorient its education-support efforts accordingly. That's one of our new thrusts here."[9]

Under Stever life at the Foundation has not been uniformly quiet. As we will have occasion to note in the next chapter, some problems have arisen as the result of action at the White House. Others—to be treated in this chapter—have erupted along the sensitive line that ties NSF to its congressional supervisors on the Hill and, by extension, to the public.

Throughout the agency's history the very nature of its work, and particularly its support of fundamental science, has made it a recurrent target of criticism. One source of these attacks has operated with such frequency that veterans of the NSF staff speak of it as "the old dilemma of the titles."

Periodically the Foundation publishes a list of the undertakings for which grants have been issued. Every year some citizens or members of the Congress and the press detect among the more abstruse titles of these projects what they believe is evidence that NSF is wasting taxpayers' money on research of a frivolous or otherwise objectionable nature. Nine times out of ten the ensuing criticism travels the same gamut. A citizen takes umbrage at some of the odder sounding titles, such as "Structure and Function of the Heart in Medieval, Medical, Philosophical and Scientific Texts," or "The Effect of the Level of Hope on the contemplation of Distance to Death," or "Hitchiking—a Viable Addition to a Multimodal Transportation System." Puzzled or outraged, or both, the citizen mails a complaint to his Congressman who, in turn, calls on the Foundation for an explanation.

At this point the process usually comes to an end. But not always. From time to time a Representative or a Senator sees fit to pass on his constituents' or his own complaints to the press—and the Foundation finds itself at the center of a round of discomfiting publicity.

Back in the early 1960's the principal congressional critic of NSF titles was Richard L. Roudebush, now head of the Veterans Administration, then a member of the House of Representatives from Indiana. At the heart of the conservative Republican's fuss with NSF was a $21,200 grant by the Foundation to enable a Colorado biologist, Keith R. Porter, to investigate the "Mating Calls and Paratid Gland Secretions" of a Central American toad. It seemed to Roudebush that the Foundation "should leave the intricacies of why toads respond to toads up to the toads themselves."[10]

Some newspaper readers agreed and said so in letters to their representatives and senators. "Isn't it about time," one argued, "that the income tax was repealed when money forcibly taken from the poor people is spent by our Government for the benefit of the very few who are interested in the sexual life of a toad?" An Oklahoma soldier wondered if "this remarkable piece of misappropriation is the reason we poor slobs in the military are having such a hard time getting a raise in pay?"[11]

But if Roudebush's press releases garnered supporters, they also garnered critics. In a 1963 radio talk to the constituents of his Ohio district, Republican Representative Charles A. Mosher deplored the "bad practice by some politicians and newspapermen [of] . . . pointing with scorn, or alarm, at the fact that the government is appropriating considerable money in support of research projects which, by title, often sound rather ridiculous. . . ." The Ohio legislator was convinced that if the disaffected journalists and politicians "would only dig a little deeper, . . . they would find that the research projects they are ridiculing . . . represent a very wise and valuable expenditure of the taxpayers' money."[12]

At the time of Roudebush's attack on the $21,200 grant to biologist Porter of Colorado, Haworth was just getting under way as NSF's second Director. Defending the scientific soundness of the grant in letters to inquiring members of Congress, he pointed out that "the taxonomic and evolutionary relationships of the Central . . . American toads (genus *Bufo)* are presently based on . . . morphology (the form and structure of plants and animals). The proposed investigation is designed to obtain nonmorphological data which will provide additional evidences as to evolutionary relationships." Haworth added that "knowledge of the chemical nature of the toad venom is currently used experimentally in heart stimulation studies. Since different species of toads vary in their toxicity, a study of variations and relationships is [medically] essential.[13]

During the 1960's most of the NSF titles brought under fire were attached to basic research projects in the natural and physical sciences. Today the bulk of the flak is being thrown at some of the social science projects and at an NSF-sponsored educational project entitled "Man: A Course of Study."

In recent years one of the Foundation's most articulate congressional critics has been Democratic Senator William Proxmire of Wisconsin, chairman of a Senate subcommittee concerned with appropriations for the Foundation. A number of the subjects which the Senator has criticized have been made to order for the press, and newspaper and magazine articles have been quick to materialize. Some of these essays, to be sure, have been a little skimpy on science, but all of them have been high in entertainment value.

One of Proxmire's pet peeves is an NSF-funded investigation by psychologist Ellen Berscheid of the University of Minnesota of the role of psychological dependency in relationships characterized by what is spoken of as "romantic love." Noting that the study poses the question, why do people fall in love?, Proxmire has remarked that "I don't want the answer."[14] Conservative newspaper columnist James J. Kilpatrick has written that he doesn't want it either.[15] On the other hand James Reston of the *New York Times* has written that he does. Pointing to the country's countless broken homes and high divorce rate, Reston has asserted that we need all of the enlightenment we can get on "the difference between infatuation and enduring love."[16]

Another project questioned by the Senator is a $135,000 probe by psychologist David Premack of the University of California at Santa Barbara of the language behavior of chimpanzees. Efforts by NSF spokesmen to set forth the validity of this one have drawn high marks from *Science* magazine and from the *New York Times Magazine*. "While the research is not mission oriented," *Science* has observed, "many scientists see hope that the results can open the way to developing new means of treatment for autistic children and for stroke victims who have lost the power of speech."[17] Writing in the June 1, 1975 issue of the *New York Times Magazine* science writer Frank Anders reported that "Therapists in a dozen American clinics and institutions have adopted a modification of Premack's chimpanzee program and applied it to the mentally retarded—and in the therapists' view it works. They are quite literally putting words in the mouths of retarded youngsters who have never spoken before. A total of 178 of 180 children who completed the program have learned to communicate; nearly 60 formerly mute retarded children are now speaking . . . and 120 others are now in different stages of training. Moreover the same program is being adapted on a limited basis for aphasic adults."

To date the Foundation's response to Proxmire has been marked by a refusal to apologize for any of its activities coupled with a prudent desire to avoid open battle with a Senator who is not only influential but who over the years has evinced approval of the Foundation's labors more often than not. Speaking as head of NSF's social sciences division, Dr. Howard Hines has endeavored to justify the studies funded by his division with the assertion that all of them are aimed at the acquisition of "a greater body of knowledge on how people interact that is scientific in nature."[18]

MACOS—to use the popular name of NSF's trouble-plagued educational project, "Man: A Course of Study"—is one of the 53 curriculum projects on the precollege level that the Foundation has helped to create and disseminate since the agency launched its first course content improvement program in 1957.* Developed with NSF money by the nonprofit Education Development Center of Cambridge, Mass., its some 65 pamphlets and color films constitute a course in human behavior for fifth-grade students. The course has been described as an introduction to anthropology that deals with one main question, "What is human about human beings?" and two subsidiary ones, "How did they get that way?" and "How can they be made more so?" One of its purposes is to demonstrate that the values cherished by one culture are not necessarily regarded in the same light by another. Small selections of the material focus on the Netsilik Indians, a society of primitive hunters in the Canadian Arctic. Filmed and printed materials indicate that the Netsiliks do not share the aversion of more advanced cultures to certain practices, such as infanticide and gerontocide.

Over $6 million and about 11 years of work went into the preparation of the MACOS series. Once ready, it was offered to commercial publishers who turned it down largely on the grounds that the expenses involved in producing the series and in training teachers to handle it ruled out the likelihood of good profits. At this point NSF, chiefly by means of a grant to the developers of the program in Cambridge, made it feasible for Curriculum Development Associates, Inc., a small private firm in Washington, D.C., to publish it. In early

---

* Discussed in chapter 8 of this book.

1975, when the argument over MACOS first reached the headlines on a large scale, the course was being taught in 1,700 elementary schools in 47 States.[19]

Initiated by Republican Congressman John B. Conlan of Arizona, the controversy over its contents and methods arose in March during a review of NSF's budget request by the House Science and Technology Committee.* In material passed out to the committee, of which he is a member, Conlan charged that the course was causing "10-year-olds and younger children . . . to reject the values, beliefs, religions and national loyalties of their parents. . . ." When a few weeks later the committee asked the NSF Director to withhold further funds from MACOS pending a congressional review of the situation, Stever obliged, an action that shortly subjected him to a charge of "censorship" by five of the Democratic members of the committee.[20]

It is reasonable to assume that the extensive publicity given the MACOS controversy was instrumental in the decision of the House of Representatives on April 9 to accept a drastic amendment to the bill covering the 1976 budget authorization for the Foundation. Introduced by Republican Representative Robert E. Bauman of Maryland, the amendment would have given the legislators an opportunity to pass judgment on all NSF grants. Happily for the Foundation—and for science—the spectacle of the United States Congress sitting as a board of review on the some 15,000 grants annually approved by the Foundation was quickly dissipated by the refusal of the Senate to go along with the Bauman amendment.[21]

In varying degrees all of the NSF Directors have found themselves coping with criticisms of the Foundation's projects. More unique in its impact on their office was the action taken by President Richard M. Nixon on January 26, 1973, when he sent to Congress a document entitled Reorganization Plan No. 1 of 1973.

Chapter 15. The Fourth Director

[1] Personal interview with Stever.

[2] *Caltech News*, July 1973, p. 3.

[3] Interview.

[4] Caltech News, ibid.

[5] *Physics Today*, May 1972, pp. 69-71.

---

* Present name of what was originally known as the Science and Astronautics Committee.

[6] To Joseph Fay, May 7, 1973, Chron File.

[7] To Richard W. Lyman, March 6, 1973, Chron File.

[8] *Priorities in Applied Research,* Report of the Committee on Public Engineering Policy of the National Academy of Engineering to the National Science Foundation, Washington, 1970, p. 19.

[9] Interview.

[10] Undated UPI dispatch. Attachment to letter, LJH to Senator B. Everett Jordan, 29 July 1963. Chron File.

[11] Letters, LJH to Senators Maurine B. Neuberger and Victor Wickersham, both attached to 14 July 1963 letter, Wickersham to LJH. Chron File.

[12] "Statement . . . for Broadcast on Sunday, October 27, 1963." Attachment to letter, LJH to Mosher, 20 November 1963. Chron File.

[13] Attachment to letter LJH to Jordan. Op. Cit.

[14] Phillip M. Boffey, "Love and Senator Proxmire," *Chronicle of Higher Education,* 24 March 1975.

[15] *Washington Star,* 27 March 1975.

[16] *New York Times,* 14 March 1975.

[17] *Science,* 16 August 1974, p. 597.

[18] Ibid.

[19] *Reston* [Va.] *Times,* 1 May 1975; *Newsweek,* 30 June 1975.

[20] "NSF Assailed for Role in Textbook Program," *Science & Government Report,* 1 April 1975.

[21] *Congressional Record,* 1 August 1975, pp. H 8063-H8077; and 30 July 1975, H 7826-H 7829.

Chapter 16

New Dimensions

Issued on January 26, Nixon's Reorganization Plan No. 1 of 1973 dismantled the science policy machinery that Eisenhower had initiated at the White House 16 years before. Out went the position of science adviser to the President, its last occupant, Ed David, having resigned on January 2. Out also went the President's Science Advisory Committee, conveniently referred to as PSAC, and the Office of Science and Technology or OST. On the heels of this scientific Gotterdammerung came the announcement from Nixon that henceforth the civilian-oriented responsibilities of the science adviser would be discharged by the Director of the National Science Foundation.

Within the scientific community early reactions to these moves were marked by an attitude of wait and see. By the time the reorganization plan took effect at mid-summer, however, opposition to the President's action was making itself heard with two themes running through the commentary. One of them was that in banishing science from the presidential presence, Nixon had "downgraded" American science itself. The other was that the "two hats" now assigned to NSF Director Stever were one too many, meaning that the President's house at 1600 Pennsylvania Avenue was a better address for science advisory purposes than NSF's headquarters at nearby 1800 G.[1] By summer's end demands that the old system be resuscitated were coming along not only from the scientists but also from the science committees in Congress—a clamor that would continue until May 22, 1975, when Nixon's successor, Gerald Ford, made known his desire to see a science advisory body once more installed at the presidential level.

A variety of factors entered into these developments; and it may be helpful in sorting them out to review some of the events connected with the origin and progress of the White House science apparatus that Nixon's reorganization plan demolished.

The notion that science should have an official voice in the Federal hierarchy has a lengthy history. What seems to have been the earliest move in this direction occurred in 1884 when the National Academy of Sciences recommended the establishment, at Cabinet level, of a

Department of Science. The ensuing debate over this proposal was sufficiently pervasive to impel a joint congressional committee to study the idea and to conclude that the scientific components of the universities "had already grown too complex for such a change in the organizational structure."[2]

During the depression era President Roosevelt turned for guidance to a temporary Science Advisory Board headed by Karl Compton, a body, as has been previously observed, that although formed by the President was not installed in the Executive Office but in the National Academy of Sciences.

During the second World War the spectacular contributions of the country's scientists and engineers to the resolution of the conflict stimulated a renewed discussion of the science-government relationship. In February 1945 Frederick G. Fassett, Jr., sometime editor of MIT's *Technology Review* and from 1945 to 1951 director of the Office of Publications at Carnegie Institution of Washington, was giving the matter considerable thought.

"One result which may be expected from this war," Fassett wrote his friend, Vannevar Bush, ". . . is the regularization and formalization of relations between the Government of the United States and science as such . . . Such an event . . . has been long overdue; because of the lack of any continuing integral relationship . . . the Nation's necessary reliance on science in times of peril has had to take the form of emergency institutions . . . . Most of the major contributory activities have a formal relationship to and part in government. Science does not, in major terms. Science (by which I mean to include technology, ,engineering, and the economic consequences thereof) perforce has remained in the state of the young man who has not attained his majority, who can fight but not vote. It is time that this sort of thing stopped.

"The way for it to be stopped is the congressional action necessary to the establishment of a Cabinet post for science. Such action would bring science into the club. It is needed if only out of fairness to science, which should be relieved of the dubious estate of being in but not of government, relieved also of the psychological danger of assuming ommniscience which is the sure result of its being ignored in easy times and turned to for the answers in times of peril"[3]

Bush had his doubts about the usefulness of a Cabinet post for science. He said as much in a reply to Fassett's letter. He argued that for the time being science should pin its hopes for a stronger tie to the Government on the labors, then well under way, that would eventuate only a few months later in the issuance of his famous report to President Truman, *Science, the Endless Frontier.*[4]

Fassett clung to his conviction that in the long pull only the establishment of a Cabinet post would suffice. Meanwhile he agreed to "hush up" until Bush's report to the President was completed and its effects became visible.[5]

A few years later the outbreak of the Korean War not only revived the issue but persuaded Truman to do something about it. Acting on the recommendations of a report prepared by William T. Golden, a corporation executive and consultant to the Budget Bureau, the President set up a Science Advisory Committee and lodged it in the Office of Defense Mobilization (ODM). In a public explanation of this step, Truman said that he wanted the members of the new unit to be where they could participate directly "in the mobilization program as it affects scientific research and development."[6]

In point of fact they did no significant participating until 1953 when an argument arose within the National Security Council at the White House over the feasibility of a proposal to discourage possible air attacks from Soviet bombers by deploying the so-called Distant Early Warning Line along the upper boundaries of North America. By asking ODM's Science Advisory Committee to assess the human and technological problems involved in this project, President Eisenhower brought a dormant organization to life.[7]

In April 1953 Vannevar Bush was in action again following receipt of an invitation to a meeting at the Executive Office to discuss a suggestion, advanced by both William Golden and Lee DuBridge, that a scientist be attached to the staff of the National Security Council. The invitation came from Robert Cutler as chairman of the staff and Special Assistant to the President for Security Affairs.

Bush did not wait for the meeting to make his views known. He wrote Cutler at once that "it would be worth while to have one of your Special Staff who at least knows the individuals in science and understands their language." He warned that there was "nothing more dangerous than to have scientific opinions interpreted by utter laymen." In addition he suggested that the National Security Council provide itself with scientific consultants by attaching the members of ODM's Science Advisory Council to the NSC staff, "where they would be readily available, and I am sure would not be in the way."[8]

Alas for Bush's hopes. Cutler himself was not averse to taking on some scientists, but the members of the NSC, generally speaking, were less than enthusiastic. On May 6 Cutler had no choice but to imform Bush that the feeling at the National Security Council was that when scientific guidance was needed it should be solicited from the National Science Foundation.[9] To which Bush replied that "I don't advocate a scientific adviser in the White House, but a regular member of your staff, as useful to you as any other member, yet having the scientific background and contacts to get you the answers you will need.

"If he is not there," Bush warned, "you may not even know when you need to ask a question. And I have seen fool things done in the White House for that very reason."[10]

And there the effort to wedge a scientific presence into the Executive Office rested until October 1957 when the public outcry at the humiliating beep-beep in the high heavens of the world's first artificial earth satellite, the Soviet Union's *Sputnik*, convinced President Eisenhower that the time had come to give science a more prominent seat in the Federal pew.

It was all done very fast. During a meeting of ODM's Science Advisory Committee on October 15, 11 days after the appearance of *Sputnik*, Eisenhower agreed with the assertion of Committee Chairman I.I. Rabi that there should be an individual in the Executive Office capable of calling the attention of the President to the "strong scientific component" found in "most of the policy matters" coming before him. The President also agreed with a couple of points offered by committee member James R. Killian, Jr., President of Massachusetts Institute of Technology.* Killian pointed out that a group "such as the Science Advisory Committee could provide proper backup for such an individual." He suggested that the committee be "given recognition and status" so that the scientist in the White House "would not be isolated from the scientific community."[11]

On November 7, in a nationally televised address, Eisenhower announced that he was creating an Office of Special Assistant to the President for Science and Technology and that its first occupant would be Dr. Killian. Two weeks later the President moved the Science Advisory Committee from the Office of Defense Mobilization into the Executive Office, thus reconstituting it as PSAC.

During the next 16 years six individuals would serve the President as science advisers: Killian, from 1957 to 1959, and Harvard chemist, George B. Kistiakowsky, 1959-61, under Eisenhower; MIT communications expert, Jerome B. Wiesner,** 1961-64, under Kennedy and Johnson; Princeton chemist, Donald F. Hornig, 1964-69, under Johnson; and Lee DuBridge, 1969-70, and Edward E. David, 1970-73, under Nixon.

---

* Subsequent to his retirement from the presidency of MIT in 1959, Dr. Killian served as chairman of the corporation until 1971, since when he has been honorary chairman.

** Now President of MIT.

When Killian took over he found Eisenhower worried over what the President spoke of as the "ill effect" of interservice rivalry on weapons research. "One of PSAC's first jobs therefore," Killian has said, "was to urge the establishment in the Department of Defense of an Office of Research and Development, a move that, I believe, strengthened the civilian hold on the Pentagon."[12]

Another of the first adviser's early discoveries was that Alan Waterman was "a little apprehensive" lest the new White House apparatus "diminish the role" of his agency. Hence Killian's decision, soon after taking over his new post, to attend a meeting of the National Science Board to make clear his desire to see the status of the Foundation "enlarged, not lessened." From this session perhaps dates the close relationship between NSF Director and Presidential Science Adviser that an astonished Budget Bureau officer would one day speak of as "so personal in nature as to defy description."[13]

The first accretion to the arrangement came in 1959 with the establishment by Executive Order of the Federal Council for Science and Technology, the only element of the White House science structure that would survive the demolition of a decade and a half later. Speaking today of the creation of FCST, Dr. Killian says that "behind our desire to set up the Council, as a forum for the heads of the major science-using agencies, was the realization that there must be at a high level in the Government a civilian-dominated science body as a counterbalance to the Department of Defense. Without such a body the Defense Department, meaning the military, is going to run the Federal science show."

Killian also looked upon the establishment of FCST as a necessary part of his efforts to achieve greater coordination among the agencies concerned with research and development. "In those days," he recalls, "various bureaus and departments were sending large amounts of money to science agencies overseas. There was a strong feeling that this program, being unmonitored, was full of duplications. FCST provided a center where the agency heads could get together and compare notes." He traces another reason for setting up the Council to a verbal report presented "at one of our early meetings" by William O. Baker of Bell Telephone Laboratories, charter member of PSAC, and a rarely conspicuous but consistently influential figure along the borderline where science and government converge. The gist of Baker's message was that "serious gaps" in the country's materials research were likely "to remain overlooked so long as no attempt was made to coordinate the research-support efforts of the various Federal agencies." An outgrowth of these revelations was the institution, at PSAC's urging, of centers for materials research, financed first by the Department of Defense, later by the National Science Foundation.[14]

The next and last addition to the President's science machinery—the Office of Science and Technology, brought into being by Executive fiat—drew much of its impetus from Congress. The year 1961 saw a succession of attempts on the Hill to strengthen the government-science relationship. Several bills appeared in the Senate. Some looked to the establishment of a Department of Science, others to the augmentation of the setup at the White House by the installation of a new working arm there. A study prepared by a subcommittee chaired by Democratic Senator Henry M. Jackson of Washington advocated the passage of a law giving permanent status to all of the science elements in the Executive Office.

Representing the interests of the President, the Bureau of the Budget was quick to take cognizance of these activities. Bureau officials saw no need for a Department of Science and frowned on proposals aimed at providing the science system at the White House with "statutory underpinning." They did see a need for enlarging it, but took the position that this should be done, not by the legislative branch, but by the President.[15]

In the spring, staffers at the Bureau began drafting plans for what about a year and a half later would become the Office of Science and Technology. These preliminary schemes reflected what seems to have been a long-held dissatisfaction on their part with the organizational structure of NSF. One of them would have changed the Foundation drastically by replacing its fixed-term Director with an Administrator, serving at the pleasure of the President, and by reducing the National Science Board to the status of a low-level advisory group.[16] By February 1962, however, the Bureau had abandoned its efforts to refashion NSF. When on March 29 John F. Kennedy established the Office of Science and Technology by issuing Reorganization Plan No. 2 of 1962, his only substantive order to the Foundation was to transfer its coordinative functions to the new organization, along with as much of NSF's policymaking functions as the director of OST would need to enable him "to advise and assist the President in achieving coordinated Federal policies for the promotion of basic research and education in the sciences."[17]

The activation of OST greatly expanded the manpower and skills available to the caretakers of science in the Executive Office. At the end of 1961 the staff assigned to the Science Adviser came to 10 professional and 13 administrative and stenographic personnel. PSAC's 18 members were aided by a small group of consultants-at-large. The Federal Council for Science and Technology consisted of the Science Adviser, representatives of eight departments and agencies having large scientific programs, and representatives of the State Department and the Budget Bureau, sitting in as observers. To these totals OST added a fulltime staff of 50 along with some 200 specialists, subsequently enlisted for part-time service on its various working panels and committees. Generation of the new unit also swelled the responsibilities of the Science Adviser. Already Chairman of PSAC and FCST, that official now became Director of OST as well.

By the time OST came along the science apparatus as a whole had already lost some of the shine that had characterized its beginnings. A man of medium height with a great stillness in his steady dark eyes, Dr. Killian today enjoys discussing his 2 years as a presidential adviser. What would become more and more of a problem to some of his successors—access to the Oval Office—was no problem to him at all.

"All I had to do to see the President," he recalls "was to check with his appointment secretary. Indeed he urged me to come see him far more than I did." According to Killian, Eisenhower took a continuous and active interest in PSAC. "The President," Killian says, "liked to meet with the PSAC members to get away for a while from the bureaucratic types."[18]

But under the iron dictate of politics the bureaucrats necessarily dominate the White House atmosphere. Essentially a generalist, the bureaucrat gives his first loyalty to the official he serves, whereas the scientist, being a specialist, gives his to the data he seeks and to the scientific community whose collective judgment determines their validity. To the bureaucrat the scientist's devotion to abstract truth often seems parochial; and the strains attendant on the presence of a group of specialists among the White House generalists began to assert themselves at an early date.

Useful in weighing the import of these strains is a recent discussion of them by David Z. Beckler, assistant to all of the President's Science Advisers, executive officer for both PSAC and OST, and now assistant to the President of the National Academy of Sciences.

"Ever since the White House science and technology mechanism was set up," Beckler writes in the Summer 1974 issue of *Daedalus: Journal of the American Academy of Arts and Sciences*, "certain forces have limited [its] . . . influence . . . on decisionmaking at the White House level. Basically, these are forces within the White House structure which tend to reject the presence of specialized points of view. The analogy of the heart transplant suggests itself. The 'alien' science and technology mechanism functions effectively only as long as the forces of rejection are continually suppressed. In the end, if the environment does not become more favorable to the acceptance of specialized advice, the transplant fails."

Largely because of the good will implicit in these attitudes the White House scientists were able at frequent intervals to influence the country's military and space programs. Reports prepared by PSAC and an OST-directed interagency task group were responsible for Presidential approval in 1969 of a "balanced program of scientific and manned space exploration and application." A 1971 PSAC study halved the estimated costs of a NASA proposal for a fully recoverable space shuttle booster and orbiter by reducing the system to a partly recoverable one. Intervention by the Science Adviser added a launch to the Apollo moon-landing series, one that greatly enlarged our knowledge of the lunar surface for a relatively small increase in the basic investment involved.

These and other accomplishments notwithstanding, the role of the White House scientists in military decisionmaking began to erode under Kennedy and deteriorated noticeably under Johnson and Nixon. Kennedy's Science Adviser, Jerome Wiesner, enjoyed the advantage of having a long and personal acquaintance with his boss. Even so Wiesner's opportunities for face-to-face dealings with the President on military matters diminished substantially after Kennedy brought in McGeorge Bundy as his Special Assistant for National Security.

To quote Beckler: "Since the President's direct involvement with science and technology issues related mostly to national security matters, the Science Adviser's lack of direct communication with the President in these areas greatly weakened his ability to bring other issues to the President. As a consequence . . . Johnson and Nixon were largely unaware of the activities and effective performance of the OST and PSAC."

Another of the "forces of rejection" listed by Beckler as operating on the White House science apparatus was the practice of all of the Presidents to regard science and technology as instruments of national policy. Beckler points out that this was especially true of "politically visible" decisions, and in retrospect it is clear that under the post-Eisenhower Presidents science's hold on the Executive Office grew steadily more precarious as it became common knowledge that many scientists, including most PSAC members, were opposed to important Administration policies.

In the September 30, 1972 issue of *SR/Science* (the separate science section of the *Saturday Review* at that time) Daniel Greenberg took note of the crumbling status of the White House science complex. Greenberg traced the decline back to the early 1960's, recalling that Jerome Wiesner, "perhaps the most influential" of the science advisers, was angered and saddened by President Kennedy's decision to resume the testing of nuclear bombs in the atmosphere.

Dr. Hornig, who followed Wiesner, soon found himself on the outs with LBJ because of the outspoken disapproval, on the part of many of his colleagues, of the Vietnam War. A story prevalent at that time and cited by Greenberg has it that Hornig served to the end of the

Administration "only because the elders of science feared that Johnson might abolish the office if the directorship became vacant."

Under Nixon new frictions emerged. Unlike all recent Presidents, he was not fond of scientists, having had occasion, during his long political career, to observe that many of them were not fond of him. On this point Greenberg quoted a recent MIT doctoral thesis entitled "Eggheads and Warheads: Scientists and the ABM." One of the relevations of the thesis was that a month prior to the 1968 election that sent Nixon to the White House a meeting of one of the PSAC panels "was converted into a working session of Scientists and Engineers for Humphrey-Muskie, to the chagrin of at least one panel member . . . a Nixon supporter . . . [who] viewed with distaste the sight of his colleagues arranging calling and canvass activities at the expense of legitimate" PSAC business.

Alarm shook the largely liberal leadership of the scientific community at a postelection report that Nixon intended to fill the Science Advisory position with Nobel laureate Willard Libby, one of the few prominent scientists to have backed the President-elect during the campaign. Presumably desirous of avoiding a fuss, Nixon gave the post instead to Lee DuBridge, who at 67 had only recently retired as President of the California Institute of Technology.

Troubles plagued DuBridge during his 20 months on the job. Not the least of them was the President's clumsily handled abortion of the appointment of ABM-critic Long of Cornell to the NSF Directorship. Another was an outgrowth of the unsuccessful effort by the Administration to win Congressional approval of plans to build a supersonic transport. At DuBridge's request, PSAC member Richard Garwin studied the SST and concluded that construction of the big plane would be economically unwise and ecologically hazardous. At the high levels of Government, unfavorable reports can be endured so long as they remain unpublished, but bureaucratic feathers flew when Garwin carried his findings to the public. "Who in the hell do these science bastards think they are!" exclaimed one Nixon aide, and the departure of Dr. DuBridge became a foregone conclusion when he found himself excluded from budgetary meetings dealing with research and development.

Dr. David, DuBridge's successor, entered office with pronounced advantages. To begin with, he was the first of the Science Advisers to come from industry instead of academia, a fact responsible for the frequent and approving assertion by President Nixon that "Dr. David is a practical man." In addition David enjoyed the blessing of William O. Baker of Bell Laboratories whose quiet effectiveness along the science-government borderline has been mentioned.

Overall David fared well. The bureaucrats welcomed him back into the budgetary sanctuary. Several of the White House scientists' substantive contributions to national policy occurred during his regime. Appropriations for research and development, previously on the decline, reversed direction. Indeed the White House science apparatus was working so smoothly at the end of 1972 that Nixon's seemingly overnight decision to throw it out caught both the country's scientists and interested members of Congress by surprise—a situation that no doubt intensified their reaction, the outpouring of protests and prophecies of technological doom that soon found their way into press.

Well in advance of the official announcement of the dismantlement in late January 1973, most of the details involved were a matter of public knowledge. NSF Director Stever was to discharge the civilian duties then being handled by the Office of Science and Technology. In addition he was to take on "the post of science adviser to the executive agencies under the President." One of his tasks, as holder of this position, would be to represent the United States in scientific ventures carried on in cooperation with the Soviet Union and other Nations. OST's military functions were to go to the National Security Council with the understanding that Stever, onetime Chief Scientist to the Air Force, would participate in such matters when occasion warranted. As for OST's coordination chores, those were to be assumed in part by the Federal Council for Science and Technology, in part by the Office of Management and Budget, and in part by Dr. Stever in his capacity as Chairman of the Council.

Whereas the White House Science Adviser had reported to the President, Stever was to report to the then Presidential Assistant and Treasury Secretary George P. Shultz. In the beginning Stever was careful to tell reporters that he would be "adviser to the government in general, not the President."[19] Only hours after this statement was made, however, the situation changed, as a result of Stever's receipt from Nixon of a letter designating the NSF head as "my adviser"—a document that former Congressman Daddario has dismissed as "vague and ambiguous" and a mere "afterthought to the notice evicting the former Science Adviser from the White House."[20]

Participants in the hue and cry gave little serious attention to the assertion by White House spokesmen that the science apparatus was only one of several agencies being demolished in accordance with Nixon's desire to streamline the Executive Office by reducing the number of employees there from 4,000 to "fewer than 2,000." In the words of the *New York Times*, the general assumption was that "an element in the Nixon decision was the President's resentment at the large number of prominent scientists who opposed him on the Vietnam war and on many other issues." Pointing out that the

President's "major problems . . . on matters of defense, environmental pollution, medical policy and the like . . . have important scientific and technical components." the *Times* editorialized that Nixon's action would "handicap him in getting first-class scientific advice quickly when he needs it." The newspaper contended that the action represented a "public downgrading" of science and technology. "Yet the United States is powerful and prosperous today," the editors asserted, "only because it has made such effective, large-scale use of scientific knowledge in all civilian and military fields."[21]

Speaking for 4,700 members, the Federation of American Scientists charged that "President Nixon, by dismantling his White House science staff, is about to lose the independent advice needed to balance his military and technical agencies."[22] A similar sentiment was voiced by the first of the President's science advisers, Dr. Killian, when on January 16, 1973, the student newspaper at MIT announced the impending dismantlement. To a visitor at his office that day Killian expressed the fear that with no civilian counterweight in the Executive Office the military would soon dominate the Federal science scene.

"I now find myself," Killian said, "almost wishing that the efforts back in the late 1950's to establish a Department of Science had succeeded. I was against that at the time, and quite frankly one reason I set out to strengthen the organization of the Government for science after becoming President Eisenhower's assistant was to head off the department idea."

In the American scientific community advocates of a Department of Science do not appear to be plentiful. Like Killian most spokesmen for the community, at any rate most of those now on record, are opposed to the centralization of Federal support for science. Most seem to feel that the availability of a variety of sources of such assistance better serves the progress of research, and particularly research of the more daring and advanced sort. Dr. Killian, when asked if he was satisfied with the way the science mechanism had developed in the Executive Office in recent years, said, "No, it's just too much at the mercy of the attitude of the Administration, and the present Administration isn't a friend. The palace guard do not like the present mechanism, and so it cannot function effectively."[23]

Not all knowledgeable observers regarded the fall of the White House science mechanism and the transfer of many of its functions to the NSF Director as an irreversible calamity. Don Price of Harvard has pointed out that there is "absolutely no use in trying to force an advisory apparatus on a President who doesn't want to use it, and so, for the time being, since I have a high respect for Guy Stever . . . I think we ought to make the best [of the new arrangement]. Similarly, in the near future, I think we should explore pluralistic devices for getting science advice . . . . You could contract for all sorts of it from different think tanks, through different channels, all reporting into headquarters. The National Security Council staff could do some of it; the Domestic Council could do some . . .; and the major cabinet officers concerned, like the HEW Secretary, could do some." Price grants that all such procedures would be "makeshifts, because I do think that advice from the major sciences on major policy issues is a general staff kind of function that belongs, not in the White House necessarily, but in the Executive Office . . ., and I think it's too bad for it not to be there on the same basis as the Council of Economic Advisers.

"But then, again," Price continues, "I don't think that the natural sciences were quite aware of the trouble they were getting into . . . by combining the function of giving scientific advice on policy with that of giving advice on policy for science." It is Price's belief that "to people in, say, the Office of Management and Budget" this mixed approach on the part of the White House scientists "made them suspect, in a way the Council of Economic Advisers wasn't, of plugging their own financial interests." He adds that "I don't think there was anything fundamentally wrong with that, but I think it made them politically vulnerable. Then when the general disenchantment of the major campuses with the Nixon Administration led to a severe split, that vulnerability was really too much vulnerability. And there was one leading Nixon Executive Office official who went back to a major campus . . ., not to Harvard, not to within 500 miles of Harvard, and he was being belabored by a group of his scientist friends as to the horrible things that had been done to science by the Administration, and he finally got a bit fed up and said, 'But you fellows don't understand the main fact—the President knows who voted for him, and he knows you didn't.' "[24]

Running through the published commentary on the dismantlement of the White House science staff was the question of how Dr. Stever could provide objective advice on the allocation of science funds among the various Federal agencies with his own agency, NSF, competing for such funds. Queried on this point by Walter Sullivan of the *New York Times,* Stever said that the same problem confronted George Shultz in his dual role as Secretary of the Treasury and member of Nixon's "supercabinet." The Foundation Director pointed out that it was Shultz's intention, as head of the newly created Council on Economic Policy in the White House, "to abstain from decisions affecting the Treasury, and that he [Stever] planned to do likewise so far as NSF was concerned." Asked about his "remoteness from the President—one level lower than the previous science advisers—," Stever said that distance was unimportant. "It's who listens that counts," he told Sullivan. The last White House science adviser, David, had seen the President, apart from large meetings, "no more than twice a month." Stever expected that Shultz would be more accessible, pointing out that he and Shultz were old friends, having known each other as young faculty members at MIT.[25]

Dr. Stever was out of the country, visiting NSF-supported installations at the South Pole, when in early December 1972 rumors of impending change in the science setup at the White House began circulating. His own knowledge of them came during a stopover in New Zealand on his way home. Reaching Washington shortly before Christmas, he encountered more rumors. In New Zealand his informants had urged him to "take a hand" in the projected changes. In Washington he was told that the changes could mean new responsibilities for the Foundation. In both instances his reaction was to point out that NSF would "stay out of politicking." His recollection now is that none of the speculations at that point comprehended the drastic nature of the changes actually in the making.

During the holiday season the NSF Director was away from Washington on vacation. The day after his return in January the last of the White House Science Advisers, Ed David, resigned; and on the 11th Stever received a call from George Shultz.

On the following day, during a morning conference at the office of the Treasury Secretary, he got the picture. "Mr. Shultz," Stever recalls, "told me that they were dismantling the whole apparatus, and that there were thoughts that I could do the science advising, and what would we do if NSF and I were asked for that."

Stever's immediate response was to schedule a planning meeting on the 14th, a Sunday, with his Deputy, Ray Bisplinghoff. On the 18th he reported back to Shultz. To the best of Stever's memory, it was during this conference that he reminded the Secretary that some of OST's duties dealt with weapons systems, and that military affairs were not the National Science Foundation's bailiwick. On the heels of this discussion the White House further refined its plan by dividing up OST's responsibilities so as to give only those of a civilian nature to the NSF Director.

Asked today if he was alarmed at the prospect of wearing two hats, Stever says no. Worry and handwringing, it seems, are not his style. Better, he says, "to get on with the job."[26]

Get on with it he did, displaying from the start an effectiveness that seems to have been lost on some of the spokesmen of the scientific community, preoccupied as they were with the itch to see science reinstated at the White House. Stever's attitude toward his chores as science adviser was that the capabilities of NSF were equal to any demands that might be put upon him in that capacity. To a West Virginia biologist, worried by the decease of OST, he wrote that "I don't believe there is occasion for distress for the following reasons: (1) Public Law 507 of the 81st Congress [the NSF enabling act] assigned science policy responsibilities to the National Science Foundation back in 1950, so there is [precedent] . . . for returning these responsibilities to the primary source; and (2) although the new responsibilities placed on NSF are enormous, the Foundation does have strengths. It has the resources that OST lacked and a long-established rapport with the science community. It has great strength also in its National Science Board. We intend to continue as many of the OST functions as have proved successful and—as has been the case in the past—we will continue to avail ourselves of the advice and guidance of the science community and its major societies."[27]

Months before OST disbanded officially on July 1, 1973, Stever had completed plans for the installation at NSF of a staff to help him carry out his advisory duties. Launched on that date, the staff was designated the Science and Technology Policy Office or STPO. Russell C. Drew, onetime head of the London branch at the Office of Naval Research and from 1966 to 1972 Technical Assistant at OST to the President's Science Adviser, was named as director. Of the performance of STPO and of other NSF staffers involved in the advisory function on ad hoc basis, Stever now says that it far exceeded his expectations. His original assumption was that "the staff and I would work up plans, and then communicate them very formally to the Executive Office. The fact is our staff has developed an inner working with many of the White House staffs."

To assist him with his advisory work Stever also set up at NSF a group known as the Office of Energy Research and Development Policy or OEP. Formally established on April 2, 1973, OEP was a fully functioning instrument when during the summer and fall of that year the United States was plunged into the most severe energy crisis of its history. Stever can be forgiven for pointing out with pride that well in advance of the Arab oil embargo, OEP was in a position to contribute substantially to the Federal Government's efforts to cope with that critical development. "If we hadn't started that one early," he says, "we would have missed a whole crisis."

Congressional concern at the abolition of the White House science staff produced two series of hearings, one in 1973 and the other in 1974. Both were conducted by what is now known as the House Committee on Science and Technology. During the 1973 hearings a committee member posed a hypothetical question. As Science Adviser and NSF Director what would Stever do if at one and the same time he were to receive a phone call from both the White

House and a big Federal agency? Stever's reply was that first of all he would find out just who in the White House was calling.

From the beginning Stever has exhibited sympathy with what he terms "the main thrust of the thinking" within the American scientific community. That thinking is that science and technology enter into many of the problems with which the White House deals. It follows that there ought to be at the President's elbow a scientist in daily contact with all of the White House staffs.

"The point to be remembered, however," he says, "is that for a considerable time after OST was discontinued, the possibility of its being resurrected was simply nonexistent. During that period my staff and I had only one way to go. Our job was to keep the flag of science flying by continuing to funnel the contributions of science and technology into the highest possible levels of the Federal Government. I think we succeeded; so much so indeed that it is my fervent hope that when science does regain the White House the President will continue to call on the National Science Foundation for the sort of inputs it is so well qualified to provide.

"When I say we succeeded," he adds, "I am saying that, among other achievements, we kept active the one element of the old White House structure to survive the fall. I refer to the Federal Council for Science and Technology. It was through that governmental coordinating mechanism, for example, that we published last year [in February 1975] the first discipline- or function-oriented analysis of the research and development aspects of the Federal budget."

The project mentioned by Stever is known officially as the "Report on the Federal R&D Program: FY 1976." Compiled by members of the Federal Council with the assistance of the Science and Technology Policy Office at NSF, it provided the Congress for the first time with a cross-departmental picture of precisely what Federal agencies are dealing with the research and development associated with such aspects of our national life as energy, climate and health. "My guess," says Stever, "is that from this day on that kind of analysis of the budget will be prepared for use by the Congress every year."

Stever's first concrete inkling that President Ford might be more receptive to first-hand science advice than his predecessor came in September 1974 when Ford invited the NSF Director to the Executive Office for a conference. "It was quite clear at that point," Stever recalls, "that the President was giving the situation serious thought. He was aware of the feeling within the scientific community that something on the order of the old White House apparatus should be inaugurated. He was also aware of similar sentiments on the Hill. He regarded the matter as a problem, and assured me of his intention of soliciting solutions to it."

A few months later Ford asked his then new Vice President, Nelson A. Rockefeller, to explore the situation and make recommendations. During this operation Stever and Rockefeller discussed the matter by phone and during a personal conference.

"It is worthy of remembrance," says Stever, "that during that exploratory phase all sorts of options were considered. They ranged from zero—that is, no official channel for science counseling—through leaving the assignment in my hands, to the creation of a new White House unit. Papers drafted for the Vice President dealt with all of the questions. Should the White House machinery, if installed, consist simply of a single adviser and a small staff, or should there be a more complete setup, a council perhaps with a larger staff? Should the adviser report, as I do, to a level somewhat below the President or directly to him? Should he handle only civil affairs or military ones as well? Should his duties be primarily budgetary or just advisory or both?"

The President was acting on the suggestions of the Rockefeller panel when on May 22, 1975, he announced his plan "to send to the Congress soon a message and proposed legislation reestablishing a science advisory mechanism consisting of a science and technology adviser to the President and a technical staff of 10 to 15 assistants." Stever's reaction, made public on the same day, was that he was "pleased that the President has taken this significant step to increase the strength of science and technology advice to the Federal Government."

Earlier in the year the National Science Board, in a letter to Rockefeller, had expressed its wish to have a strong voice for science at the presidential level. Stever's recollection is that "the Board really didn't move in that direction much." It would appear that its message to the Vice President, never made public, offered no suggestions concerning the form and scope of the hoped-for apparatus.

Issued over the signature of Chairman Norman Hackerman, the Board's comment on the President's move of May 22 read as follows:

> The National Science Board welcomes the announcement of the President's plan to submit legislation to establish a new science advisory structure in the White House. The Board is pleased that the President has decided to establish a mechanism suited to his style. We hope this new arrangement will ensure that the most effective scientific input is available to him in his policymaking role and to the Executive Branch in its formulation of science policy and the allocation of resources for science and technology.

> The National Science Board looks forward to cooperating with the new science advisory staff. We will assist in every possible way to enhance the ability of the President to draw on the versatility of science and technology as he tackles national issues.
>
> Dr. Stever has kept the Board informed of his activities as Science Adviser, and we believe he has discharged these duties in a commendable manner. On behalf of the scientific community we express our appreciation to him.[28]

At this writing congressional response is in the form of two bills, one roughly in line with the President's specifications and sponsored by Democrat Olin E. Teague of Texas, chairman of the Science and Technology Committee, and Mosher of Ohio, ranking Republican member. Another bill, similar in many ways but carrying a number of additions not desired by the White House, has, under the leadership of Senator Edward M. Kennedy, been passed by the Senate. The eventual compromise that will emerge from the Committee of Conference cannot now be predicted. It is a foregone conclusion, however, that in time a bill setting up a White House advisory body will reach the President's desk.

Asked how the projected apparatus will fare, Stever ventures a quick and knowing smile. "That," he says, "is the key question." He points out that not all of the resident staffs at the White House are going to welcome the proposed unit, some of them regarding the return of the scientific specialists as a threat to "their turfs."

It is obvious—at least to this writer—that some reservations lay behind the NSF Director's sportsmanlike endorsement of the President's proposal to move the science advisory machinery out of NSF and back to the White House.

Stever concedes that the present advisory arrangement puts the scientific community at a disadvantage in that it has no representative close to the President on a continuous basis. "On the other hand," he says, "I think the experience of the last few years has shown that the present arrangement has one great advantage. Take some of our tougher and more stubborn national problems. Thanks to the great resources accessible to NSF, we can dig into those far more thoroughly than any White House office can. In this connection I call your attention to our function-oriented analysis of the Federal R&D budget last year. I also call your attention to our preparation and issuance, in June 1975, of the so-called IMOS report."

The full name of the IMOS report is "Fluorocarbons and the Environment: Report of Federal Task Force on Inadvertent Modification of the Stratosphere (IMOS)." As those designations indicate the report deals in detail with the effect of aerosol sprays and other entities on the ozone found at a layer of the terrestrial atmosphere where it protects the inhabitants of the earth by absorbing much of the more harmful radiation emitted by the sun.

When Stever is reminded that some prominent members of the scientific community have called him the logical person to fill the upcoming advisory post at the White House, he allows himself another quiet smile.

"Well, you know," he says, "I don't even comment on that, obviously. I haven't been offered the job. I have a job, and if I'm offered that job they'll have to fill this one. It would be interesting to do, but I don't cross that kind of bridge until I come to it."

On that not unprovocative note, let us conclude—or more exactly, suspend—this chronicle of some of the highlights of the first 25 years of a lively, unusual, troubled, fast-growing, and useful Federal organization.

Chapter 16. New Dimensions

[1] William O. Baker in *Chemical and Engineering News*, 14 August 1975.

[2] Jerome Wiesner, *Where Science and Politics Meet*. New York, 1964, p. 56: Vannevar Bush, memo to John Teeter, April 11, 1947, Bush Papers, LC (Box 85, file 1912).

[3] Fassett to Bush, February 6, 1945, Bush Papers, LC (Box 37, file 886).

[4] Bush to Fassett, February 9, 1945, ibid.

[5] Fassett to Bush, February 27, 1945, ibid.

[6] David Z. Beckler, "The Precarious Life of Science in the White House," *Daedalus*, Summer 1974, pp. 117-118 (cited hereafter as Beckler); NAS, *Science and Technology in Presidential Policymaking: A Proposal*. Report of the ad hoc Committee on Science and Technology, Washington, June 1974, p. 15.

[7] Beckler, p. 117.

[8] Bush to Cutler, April 14, 1953, Bush Papers, LC (Box 30, file 678).

[9] Cutler to Bush, ibid.

[10] Bush to Cutler, May 7, 1953, ibid.

[11] Beckler, pp. 117-118.

[12] Personal interview.

[13] Seidman, memo to Staats, November 16, 1961, BOB 61.5 (E8-21/61), OMB Records Division.

[14] Interview with Killian.

[15] BOB 61.5 (E8-23/61.1) 1961-65, and (E8-23/62.1) 1961-62. OMB Records Division.

[16] "Reorganization Plan," May 18, 1961, BOB 61.5 (E8-24), ibid.

[17] BOB 61.5 (E8-23/62.1), ibid.

[18] Interview.

[19] *Washington Post*, January 27, 1973.

[20] Emilio Q. Daddario, "Science Policy: Relationships Are the Key," *Daedalus*, Summer 1974, p. 137.

[21] *New York Times*, February 7, 1973.

[22] *Washington Post*, January 27, 1973.

[23] Interview.

[24] Personal interview with Price.

[25] *New York Times*, January 31, 1973.

[26] Interview.

[27] To Earl L. Core, June 1, 1973, Chron File.

[28] Memo, Vernice Anderson to Members of NSB, 2[illegible] May 1975, and attachments. NSB files.

Notes

Abbreviations

AR	Annual Report
ATW	Alan T. Waterman
BOB	Bureau of the Budget Papers
Chron File	A daily file maintained in the O/D
HGS	H. Guyford Stever
LC	Library of Congress
LJH	Leland J. Haworth
NA	National Archives
NSB	National Science Board
NSB Min	Minutes of meetings of the NSB
NSB-ES Min	Minutes of executive sessions of the NSB
NSF	National Science Foundation
O/D	Office of the Director, NSF
OGC	Office of the General Counsel, NSF
OMB	Office of Management and Budget
WDM	William D. McElroy

Index

Aberle, Sophie B., 65
Adams, John Quincy, 11
Adams, Roger, 66
Aerojet-General, 175
AEC. See Atomic Energy Commission
Allott, Sen. Gordon, 176, 180, 224, 227
Alpert, Harry, 205
American Association for the Advancement of Science, 49-53, 91fn.
American Miscellaneous Society (AMSOC), 169-177 *passim*
AMSOC Committee. See American Miscellaneous Society
Anders, Frank, 257
Applied science. See National Science Foundation, applied-science support
Arecibo. See National Astronomy and Ionosphere Center
Associated Universities, Inc. (AUI), 116, 118, 141, 143, 145, 146
Atomic Energy Commission, 45, 48, 67, 102, 103, 116, 117
Ashton, Ned L., 143
Baker, William O., 179, 265, 269
Baldwin, Ira L., 66
Barnard, Chester I., 65, 66, 105, 106
Barnes, Robert P., 30
Bascom, Willard N., 169, 170, 176
Basic science. See National Science Foundation, basic-research support
Battelle Columbus Laboratories, 13
Battelle Memorial Institute, 162
Bauman, Rep. Robert E., 259
Beckler, David Z., 267, 268
Belcher, Donald R., 106
Bell, Jocelyn, 9
Beloussov, Vladimir V., 191
Bence-Jones, Henry, 19
Benson, Ezra Taft, 101
Benson, William E., 4, 168, 178, 181, 191
Berkner, Lloyd, 65, 66, 109
Betscheid, Ellen, 257
Bisplinghoff, Raymond L., 232, 243, 244, 246, 273
Bowman, Isaiah, 47, 48, 49, 51, 86, 205
Bronk, Detlev W., 29, 64, 65, 73, 80, 81, 105, 106, 108, 173, 185
Brookhaven National Laboratory, 116
Brooks, Harvey, 134
Brown, George R., 176, 179
Brown & Root, Inc., 174, 176, 178-184, 186-188, 190
Buchta, J.W., 122, 123
Bureau of the Budget, 43, 55, 56, 63, 91-110 *passim*, 132, 183, 184, 189, 206, 209, 218, 226-228, 232, 234, 237-241, 248, 249, 266, 270
Bureau of Labor Statistics, 15, 16
Bush, George W., 179
Bush, Vannevar, 36-38, 40, 41, 43, 49-51, 56, 62-64, 66, 67, 112, 113, 200, 205, 262-264
Cabell, Rep. Earle, 248, 249
Carey, William D., 91-110 *passim*, 208, 210, 211
Carmichael, Leonard, 100, 101
Carter, Herbert E., 247
Cerro Tololo Inter-American Observatory, 148
Challkey, Lyman, 57, 112
Coggeshall, Lowell T., 66
Columbia University, 4, 112
Committee on Public Engineering Policy, 245
Committee Supporting the Bush Report, 49, 50, 52
Compton, Karl T., 35, 51, 68, 72, 261
Conant, James B., 36, 37, 38, 42, 46, 47, 64-66, 68, 69, 73, 81, 82, 121
Congress. See National Science Foundation, relations with
Conlan, Rep. John B., 259
Connick, Robert E., 158, 159
Consolazio, William V., 244-246
Continental drift, 3, 4
COPEP. See Committee on Public Engineering Policy
Cori, Gerty Therese, 29
Cornfield, Jerome, 16
Cornell University, 7
Cowen, Robert C., 187
Cox, Oscar S., 41, 42
Creutz, Edward C., 231
Crosser, Rep. Robert, 57
Curriculum Development Associates, Inc., 258
CUSS I, 170, 171
Cutler, Robert, 99, 263
Daddario, Emilio Q., 114, 164, 199, 200, 202, 213, 215, 219, 220, 221, 222, 223, 270
Daddario bill, 199-213 *passim*
Daddario subcommittee. See House Committee on Science and Technology
David, Edward E., 241, 261, 264, 269, 270, 273
Day, Edmund E., 52, 56
Deep Sea Drilling Project, 191fn., 195
Department of Health, Education, and Welfare, 44, 66, 102, 103, 105, 106, 229, 240
Dewey, John F., 5
DeWitt, Nicholas, 122
Dodge, Joseph, 98, 100

Dollard, Charles, 80
Drake, Charles L., 2, 3
Drake, Frank D., 7, 8, 10, 11
Drew, Russell C., 274
Dryden, Hugh L., 99
DuBridge, Lee A., 31, 104, 221, 222, 241, 263, 264, 269
Dupree, A. Hunter, 34
Durkin, Martin P., 101
Dyson, Freeman J., 23, 26
Eddington, Arthur, 21
Edelman, Gerald M., 16-18
Eggers, Alfred J., Jr., 247
Einstein, Albert, 20, 23
Eisenhower, Dwight D., 99, 103, 125, 179, 222, 264, 265, 267, 268
Elberg, Sanford S., 163
Ellender, Sen. Allen J., 81
Embrey, Lee Anna, 77
England, J. Merton, 131
Estabrook, Frank B., 168
Evans, Duane, 16
Evins, Rep. Joe L., 180, 190
E. W. Bliss Company, 143-145
Fassett, Frederick G., Jr., 262, 263
Federal Council for Science and Technology (FCST), 108, 132, 266, 265, 266, 267, 270, 275
Federation of American Scientists, 271
Federation of American Societies for Experimental Biology, 222
Fellowships. See National Science Foundation, Education-Support
Field, John, 77
Flemming, Arthur S., 99
Folsom, Marion B., 107
Ford, Gerald, 261, 275
Francisco, Enos, 148
Franklin, Benjamin, 3
Fulbright, Sen. J. William, 240
Garwin, Richard, 269
"Gas Company" (Global-Aerojet-Shell), 177-180
General Electric, 177
General Motors, 174
Geological Revolution, 2-7
GeoTimes, 182
Gerard, Ralph W., 111
Gilliland, Edwin R., 130
Giraud-Soulavie, Abbe J. L., 3
Global Marine Exploration Company, 170, 175
Glomar Challenger, 195
Golden, William T., 263
Gordon, Kermit, 183
Fould, Laurence M., 148
Gravitational waves, 20-27
Greenberg, Daniel S., 97, 163, 164, 186, 200, 268, 269
Greenwood, Gerald O., 147
Griswold, A. Whitney, 125
Gustavson, Reuben G., 66
Hackerman, Norman, 276
Hafstad, Lawrence R., 96
Hagerty, James C., 104
Handler, Philip, 163, 186, 187, 212, 219, 223, 248
Harrison, George L., 45
Harvard University, 36
Harwood, Wilson E., 79
Hastings, A. Baird, 65, 66
Huage, Gabriel, 99
Haworth, Mrs. Irene, 117
Haworth, Leland J., 113-119, 128, 129, 133, 134, 154, 157, 158, 161-164, 168fn., 184, 187-190, 192, 202, 208, 210-212, 215-218, 221, 227, 256
Haworth, Paul, 116
Hedberg, Hollis D., 175, 181, 182, 185
Henkin, Leon A., 159, 160
Heroy, Walter B., Sr., 170, 173
Heselton, Rep. John W., 51, 95
Hess, Harry H., 3, 5, 168
Hesson, W., 160
Hewish, Anthony, 8
Heyns, Roger W., 163
Hill, Sen. Lister, 208, 212
Hines, William, 162
Hobby, Oveta Culp, 105-107
Hoff, William Jay, 104
Hoffenberg, Marvin, 16
Hopkins, Harry, 37, 41, 42
Hornig, Donald F., 210, 264, 268
House Committee on Merchant Marine and Fisheries, 185
House Committee on Science and Astronautics. See House Committee on Science and Technology
House Committee on Science and Technology, 114, 164, 199, 202, 203, 206, 211, 219, 247, 249, 274
House Un-American Activities Committee (HUAC), 154, 155, 157
Hughes, Rowland R., 105-107
Humphrey-Muskie, Scientists and Engineers for, 269
Humphreys, Lloyd G., 232, 248
Hystad, Carlyle E., 237, 238, 241
ICSRD. See Interdepartmental Committee on Scientific Research and Development
I-EE University Well, 170
Immune system, 16-20
Industrial and Engineering Chemistry, 30
Infeld, Leopold, 23
Input-Output technique, 12-16
Interdepartmental Committee on Scientific Research and Development, 94-97
Interdisciplinary Research Relevant to Problems of our Society (IRRPOS or IRPOS), 218, 220, 221, 232, 238, 241-244
International Congress of

Mathematicians (SCM), 152
International Geophysical Year (IGY), 137
Inter-Society Committee for a National Science Foundation, 52, 53
IRPOS. See Interdisciplinary Research Relevant to Problems of Our Society
IRRPOS. See Interdisciplinary Research Relevant to Problems of Our Society
Isacks, Bryan, 6
Jansky, Karl, 140
J. C. Fields medal, 153
Jerne, Niels K., 17
Jewett, Frank B., 46
Johns Hopkins University, 29, 47
Johnson, Lyndon B., 134, 179, 199, 264, 268
JOIDES. See Joint Oceanographic Institutions for Deep Earth Sampling
Joint Oceanographic Institutions for Deep Earth Sampling, 192-195
Joule, James Prescott, 144fn.
Journal of Experimental Medicine, 19
Karmanov, Vladimir, 156
Kelly, Harry C., 76, 86, 122, 123
Kelson, Keith R., 127
Kennedy, Sen. Edward M., 199fn., 213, 219, 222, 277
Kennedy, John F., 116, 206, 264, 266, 268
Kevles, Daniel J., 41, 42
Keynes, John Maynard, 15
Kilgore, Sen. Harley M., 39-41, 43, 45, 48, 112
Kilgore bill (S. 702), 40, 41, 42
Kilgore-Magnuson compromise (S. 1850), 49-51
Killian, James R., Jr., 264, 265, 267, 271
Kilpatrick, James J., 257
Kinzel, A. B., 144
Kistiakowsky, George B., 114, 264
Kitt Peak National Observatory, 148
Klopsteg, Paul E., 77
Koepfli, Joseph B., 76
Krebs, A. W., Jr., 78
Krieghbaum, Hillier, 124, 125
Kuchel, Sen. Thomas, 180
Kuhn, Thomas S., 27
Larnum, Russell R., 102, 103
Lehman, Sen. Herbert, 82
Lennon, Rep. Alton A., 185, 187, 189
Leontief, Wassily W., 12, 14, 15
Levin, Louis, 132, 232
Libby, Willard, 269
Lill, Gordon, 169, 175, 192
Long, C. N. H., 66, 107
Long, Franklin A., 201, 221, 223, 269
Long report; See Department of Health, Education, and Welfare
MACOS. See Man: A Course of Study
Magnuson, Sen. Warren G., 43, 45, 48
Magnuson bill (S. 1285), 43
Man: A Course of Study, 257-259
Mansfield Amendment, 240
Mansfield effect, 240
Mansfield, Sen. Mike, 240
Manza, August, 159
Massachusetts Institute of Technology, 9, 36, 65
Maverick, Maury (former Representative), 47, 48
Meyerhoff, Howard A., 49-51
Miller, Rep. George P., 164, 202, 203, 211, 247
Millikan, Robert A., 251
Mills, Rep. Wilbur D., 50
Mohole. See Project Mohole
Moho. See Project Mohole
Mohorovicic discontinuity. See Project Mohole
Moreland, Edward L., 65
Morris, Joseph C., 79, 81
Morse, Marston, 30, 65
Morse, Wayne, 127
Mosher, Rep. Charles A., 247, 256, 277
Munk, Walter H., 168
Murphy, Walter J., 30, 31
MV Caldrill, 194
McBirney, Alexander R., 2, 3
McCarn, Doris, 72
McCarthy, Sen. Joseph R., 68
McElroy, Mrs. Marlene, 223, 224
McElroy, William D., 223-231, 233, 241, 242, 244-247, 249
McKellar, Sen. Kenneth D., 81, 82
National Academy of Sciences, 35, 106, 137, 153, 155, 159, 161, 169, 172, 186, 187, 223, 248, 261, 262
National Advisory Committee for Aeronautics (NACA), 36, 99
National Aeronautics and Space Administration (NASA), 63, 137fn., 172, 229, 240
National Astronomy and Ionosphere Center, 7, 149
National Center for Atmospheric Research (NCAR), 147
National Defense Education Act, 138
National Institutes of Health. See Department of Health, Education, and Welfare
National Research Council. See National Academy of Sciences
National Radio Astronomy Observatory, 9, 139-148
National Science Board, 1, 2, 61, 62, 79, 81, 86, 105, 107, 112, 115, 118, 122, 123, 131, 133, 182, 184, 186, 189fn., 207, 210, 212, 213, 219, 221, 223, 227, 228, 241-243, 246, 248, 249, 265, 276, 277
  first meeting, 29-32, 64, 65
National Science Foundation, 1, 2, 79, 209, 210, 212

  advisory committees and panels, 74, 168, 216
  and coordination of federal research activities, 31, 73, 91-110 *passim*, 206
  and executive orders, 97-108
  and national problems, 114, 119, 201, 202, 204, 217-221, 243
  and national science policy, 31, 73, 82, 83, 206, 207, 212, 266
  and Presidential reorganization plans, 108, 206, 259, 261, 266
  and peer review, 74, 168
  applied-science support, 200, 201, 204, 208, 209, 211, 215-221, 237-250 *passim*, 254
  basic-research support, 1-27 *passim*, 30 82-85, 99, 103, 111, 112, 113, 114, 204, 211, 216, 227, 238, 239
  budgets, 1, 67, 79-82, 98, 113, 134, 201, 204, 218, 219, 221, 226, 233, 234, 238, 239, 246, 249, 254
  education-support, 82, 85-88, 121-130, 226, 237, 239, 248, 249, 257-259
  homes, 78, 79, 91
  information programs, 138, 226
  institutional programs, 130-135, 241, 244, 248, 249
  international programs, 137, 210, 270
  origins and legislative history (see also Daddario bill), 33-57 *passim*, 73-74, 94, 95, 114
  publications, 122
  relations with Congress, 30, 79-82, 111, 114, 118, 124, 157, 201, 219, 227, 247, 255-259
  relations with government agencies, 73, 75, 86, 91-110 *passim*, 180, 201, 207
  relations with industry, 115, 200, 204, 227, 254, 255
  relations with news outlets, 111, 112, 156, 167, 256
  relations with White House, 97-108, 212, 213, 221-223, 232, 254, 261, 273-276
  social-science support, 204, 205, 206
  structure, 30, 54, 55, 74, 231
  staff, 73, 74-78, 118, 212, 216, 224, 227, 229, 231
  weather modification, 137, 211
National Science Foundation: Its Present and Future, 203, 206
National Sea Grant College and Program Act, 139
National Steel and Shipbuilding Company, 190
Naval Research Laboratory, 229
Netsilik Indians, 258
Neutron stars. See Pulsars
New York Herald-Tribune, 81
New York Times, 31, 81, 86, 111, 116, 117, 134, 222, 257, 270, 271
Nixon, Richard M., 215, 221, 222, 224, 237, 259, 261, 264, 268-271
NSB. See National Science Board
NSF. See National Science Foundation
O'Brien, Francis G., 134
Ocean Science and Engineering, 176, 177
Ocean Sediment Coring Program, 189fn., 192
Office of Management and Budget. See Bureau of the Budget
Office of Naval Research, 67, 72, 175
Office of Science and Technology (OST), 206, 207, 244, 261, 266-268, 273
Office of Scientific Research and Development (OSRD), 36, 38-41, 44, 66, 67, 108, 116, 179
Oliver, Jack E., 4-7, 192
Oppenheimer, J. Robert, 8, 23, 252
Owen, Thomas B., 232
Oxford University, 16
Pace, Frank, Jr., 56
Pell, William H., 160
Penick, James L., Jr., 63
Phillips, Rep. John, 80
Physics Today, 22
Piel, Gerard, 16
Piore, Emanuel R., 184, 221
Politics, Science & Dread Disease, 95
Plate tectonics, 3-7
Porter, Keith R., 256
Porter, Rodney R., 16, 18
Premack, David, 257, 258
President's Science Adviser, 114, 206, 261, 266, 267
President's Science Advisory Committee (PSAC), 114, 130, 133, 206, 207, 261, 263-265
Price, Don K., Jr., 37, 108, 202, 211, 271
Priest, Rep. James Percy, 54, 56, 94
Princeton University, 23
Project Mohole, 167-193, 224
"Prologue to the U.S. National Science Foundation," 57, 112
Proxmire, Sen. William, 257, 258
PSAC. See President's Science Advisory Committee
Public Health Service, 44, 83
Pulsars, 7-11
Quesnay, Francois, 13
Rabi, I. I., 264
Radiation Laboratory, 116
Radio Astronomy. See National Radio Astronomy Observatory
Ramspeck, Robert, 75
RANN. See Research Applied to National Needs
Rawson, Hugh, 124, 125
Reber, Grote, 140, 141fn.
Reifenstein, Edward C. III, 9, 10
Research Applied to National Needs, 237-250 *passim*, 254
Reston, James, 257

Reyniers, James B., 29
Reynolds, William E., 78
Rice, Donald B., 241
Richards, A. N., 38
Robertson, Randal M., 182, 217, 218, 219, 220, 221
Rockefeller, Nelson A., 106
Rockefeller University, 16, 18
Roosevelt, Franklin D., 35, 37, 41, 42, 43, 262
Rosenman, Samuel I., 42
Rosenthal, Aaron, 182
Roudebush, Richard L., 161, 162, 164, 256
Rubey, William W., 182
Ruebhausen, Oscar M., 41, 42
Ryer, David E., 226, 228, 241, 245
Saltonstall, Sen. Leverett, 49
Scherer, Paul A., 181
Schimmel, Herbert, 41, 51, 112
Schlesinger, Arthur M., Jr., 100
Schuldt, Frederick C., 87
Schultz, Charles L., 168fn
Schwartz, Leon M., 247
Science, 21, 49, 50, 163, 177, 186, 222, 230, 257
Science, attitudes toward, 202
Science Adviser. See President's Science Adviser
Science Advisory Board, 35, 262
Science and Public Policy, 54, 63, 94
Science policy. See National Science Foundation and national science policy
Science Policy Research Division, Library of Congress, 203
Science, the Endless Frontier, 43, 63, 86, 205, 262
Scientific American, 16
Seaborg, Glenn T., 133
Seitz, Frederick, 185, 208
Shapley, Harlow, 52
Shell Oil, 180
Shepherd, William G., 216
Sheppard, Frank C., 181
Shultz, George P., 270, 273
Sides, Virginia B., 77, 84
Sisco, Bernard, 229, 230, 231, 241, 242fn
Smale, Stephen, 151-164 *passim*
Small, Maxwell M., 146, 147
Smith, Sen. H. Alexander, 52
Smith, Harold D., 47, 55
Smith, Homer W., 50
Smyth, Henry D., 103
Snow, Joel A., 219, 220, 232, 243-247
Socony Mobile, 174, 176 177, 178, 179, 180
Staats, Elmer B., 55, 95
Staelin, David H., 9
Steelman, John R., 54, 62-64, 67, 68, 94, 96
Steelman Report. See *Science and Public Policy*
Stever, H. Guyford, 221, 249-255, 261, 270-275, 277
Stewart, Irvin, 38
Stone and Webster, 146
Strauss, Lewis L., 102
Strickland, Stephen P., 95
Subcommittee on Science, Research and Development. See House Committee on Science and Technology
Sunderlin, Charles E., 76, 84
Teague, Rep. Olin E., 277
Teeter, John H., 51
Texas Instruments, 174
Thomas, Rep. Albert, 80, 111, 134, 179, 190
Thomas, Sen. Elbert D., 52, 53, 54
Titles dilemma, 255-257
Toledano, Ralph de, 162
Truman, Harry S., 29, 30, 43, 45, 53, 54, 57, 62-69, 94, 96, 109, 111, 263
Trytten, M. H., 86, 87
Tuve, Merle A., 141
Wadsworth, Rep. James W., 56
Walker, Eric A., 208
Walsh, John, 230
Waterman, Alan T., 55, 65, 66, 68, 69, 71-83, 87, 91-110 *passim*, 111, 112, 117, 118, 121, 124-126, 131, 142, 144, 148, 173-175, 177-182, 184, 192, 193, 202, 224, 227, 240, 265
Waterman, Frank Alan, 71
Waterman, Mrs. Mary, 69, 77, 78
Watson, James D., 20
Weather modification. See National Science Foundation
Weaver, Warren, 144
Webb, James E., 63, 109
Weber, Joseph, 21-27
Wheeler, John A., 23
White House. See National Science Foundation, relations with
Wiesner, Jerome B., 264, 268
Wilson, Charles E., 29
Wilson, John T., 107, 118
Wolfle, Dael, 53, 55, 56
Woodward, C. Don, 181
Wyckoff, P. H., 138
Zacharias, Jerrold R., 125
Zapata Off-Shore Company, 175, 177, 179
Ziman, John M., 138

U. S. GOVERNMENT PRINTING OFFICE : 1976 O - 216-388